JUST A BEGINNING: COMPUTERS AND CELESTIAL MECHANICS IN THE WORK OF WALLACE J. ECKERT

by

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This thesis details and analyzes the interaction between computers and science in a particular case. The case is the career of Wallace J. Eckert (1902-1971). Eckert was a professor of astronomy at Columbia University and scientific researcher for IBM. He has received some attention in the history of computing for his significant work in machine computation in the 1930s and 1940s and was the foremost expert on lunar theory for much of his life.

First the existing secondary literature on the subject is discussed. Eckert’s work has rarely been the focus of sustained historical scrutiny, but the question of the relation of science and the computer has received more scholarship in the history, philosophy and sociology of science. The main narrative of the thesis begins with the history of the various mathematical techniques and external aids to computation used over the course of the history of celestial mechanics. Having set the context, Eckert’s early life and career is detailed up until 1945. Here, before the modern computer as such was developed, Eckert innovated by adapting IBM punched card machines to astronomical applications. Next Eckert’s time as a scientific researcher employed by IBM after 1945 is detailed. Here he helped establish a culture of scientific research at IBM, demonstrated the value of IBM’s products for science, aided in the development of new more complex machine designs including electronic systems and continued his own astronomical research. Eckert’s major projects on electronic machines are described, especially those in lunar theory, with
explanation of how his astronomical methods remained the same or were modified and expanded by later electronic machines and how he innovated with the machines at his disposal.

In the conclusion, after summarizing later developments in celestial mechanics, broader questions about the modern computer’s role in science are engaged. Continuity between pre and post computer methods is well illustrated by Eckert’s work. His work also shows that while the computer was a force for change in celestial mechanics, the form of that change depended on the choices, resources and practices of the people using it.
Dedication

Dedicated to my parents without whose support this thesis might never have been written and the memory of Herb Grosch who helped cement my interest in Wallace J. Eckert.

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Chapter 1

Introduction

Astronomy is perhaps the oldest of the exact sciences. Starting from the naked eye, the tools of the observational astronomer have multiplied over the course of history, from gnomon and simple compasses to massive telescopes, spectroscopes and radio antenna. However astronomical theory lacked this accretion of material culture for most of the discipline’s history. The pen and paper seemed to be all that was needed for the theoretician’s craft. A closer inspection reveals that their work has adopted various aids to computation that augmented and changed the work. The invention of writing and mathematical notation itself was the first material aid to human computation, eventually mathematicians wrote multiplication tables and tables of other functions. Tables of chords, sets of numbers for use in calculation, precursors of later trigonometric tables, appear in the classic work of ancient Greek astronomy Ptolemy’s (c.90-c.160) *Almagest*, in the 2nd century of the Common Era (C.E.). (Ptolemy 1952, 21-24) Eventually mechanical calculators would be used to ease the researchers labours and finally calculations would be fully automated with the advent of the modern computer.

Since at least the fifth century B.C.E. in Seleucid Babylon, attempts have been made to predict, through calculations, the motion of the planets and the Moon. (Neugebauer 1962, 102) With the emergence of the Newtonian theory of gravity and calculus, calcu-
lations became based on inductively derived universal physical principles, rather than the attempt to find particular cyclic patterns in past observations. However, solving the equations of motions for more than two bodies under gravitational force yields no finite solution. The methods required to determine an approximation of a celestial body’s motion are complex and arduous. (Linton 2004, 295-298) Astronomical calculations were a fertile ground for innovation including automation by machine.

Wallace J. Eckert (1902-1971), Columbia University astronomer and IBM researcher, was one of the first to automate astronomical calculations. He tackled problems in celestial mechanics, first with IBM punched card accounting machines in the 1930s, then in 1948 with IBM’s first large scale general purpose electronic machine the Selective Sequence Electronic Calculator (SSEC). Eckert would continue to use IBM’s computers to solve problems in celestial mechanics up until his death in 1971. These efforts led to his recognition as both a pioneer in the field of computers and an important figure in celestial mechanics.

Mainstream histories of computing note Eckert’s importance. For example, Herman H. Goldstine (1913-2004) called Eckert “perhaps the greatest modern figure in numerical astronomy.” (Goldstine 1993, 109) Goldstine also credited Eckert’s collaboration with IBM as developing a flexibility at IBM towards scientific customers that may have been key to their future success in electronic computers. (Goldstine 1993, 110) Despite this high praise, Goldstine failed to describe Eckert’s actual work in astronomy beyond a brief mention of his use of IBM’s SSEC for improving on E. W. Brown’s (1866-1938) tables. The omission is surprising given Goldstine’s review of work in lunar theory leading up to Eckert’s own (Goldstine 1993, 27-30, 107-109, 327). Eckert’s work has not been much studied or reviewed by historians beyond brief mentions in broader histories and entries in encyclopedic reference works.

An example of a briefer account of Eckert’s work is the Dictionary of Scientific Biography entry by Henry S. Tropp (1927-2007). The particulars of Eckert’s life and work
are given, in six paragraphs. Tropp briefly outlined Eckert’s work starting with his use of punched card machines in the 1930s. He mentioned Eckert’s work as director of the Nautical Almanac Office (NAO) from 1940-1945. He then focused on Eckert’s early work as the head of IBM’s Pure Science department and director of the Thomas J. Watson Scientific Computing Laboratory developing and designing the SSEC and the Naval Ordnance Research Calculator (NORC). Finally, Tropp discussed Eckert’s extensive work in lunar orbit theory. However, the only specific work by Eckert, referred to by Tropp, was the computation of the positions of the five outer planets on the SSEC. Tropp failed to mention that Eckert worked with computers other than the SSEC and NORC. However, the entry does contain an extensive and useful bibliography. (Tropp 1978, 128-130) Tropp’s entry is representative of how Eckert is treated in historical literature with a focus on his pioneering work on calculating machines in the 1930s and 40s and little interest in the details of his astronomical work, despite emphasizing its importance.

One exception to this trend is an article, for a sesquicentennial anniversary conference on the NAO, by Martin Gutzwiller (1925-), a former colleague of Eckert’s at the Watson Laboratory. Here Gutzwiller gives a fairly comprehensive survey of Eckert’s work. Gutzwiller touches on Eckert’s early work with punched card machines, his work at the NAO, his work for IBM and role in developing computers for them and some of Eckert’s lesser innovations such as automation of the taking of stellar positions from photographic plates. However, Gutzwiller spends a great deal of time detailing Eckert’s work on lunar theory. Gutzwiller begins with Eckert’s attempts to verify and correct E. W. Brown’s work in the 1930s. He then details Eckert’s calculations of lunar positions from Brown’s theory on the SSEC and his later improved lunar theory done in collaboration with Harry F. Smith (1929-) and Rebecca Jones (?-1966). In fact Gutzwiller helped Eckert’s colleague Sara Bellesheim continue with Eckert’s final attempt at a new lunar theory after his death. (Gutzwiller 1999, 147, 150-159) Gutzwiller’s brief account is unique for the amount of attention it pays to the specifics of Eckert’s scientific work. Gutzwiller also
manages to lay out in a more proper proportion the various activities Eckert undertook in his career.

The longest work that concerns itself mainly with Eckert’s career is Jean Ford Brennan’s *IBM Watson Laboratory at Columbia University: A History*. This 50 plus page monograph deals with both the Lab and its prehistory. It begins with the appearance of a punched card laboratory at Columbia under the auspices of Professor Ben Wood (1892-1984), who convinced IBM President Thomas J. Watson Sr. (1874-1956) to supply machines free. It was through these machines that Eckert was able to experiment with the use of punched card machines for complex scientific calculations. As Brennan explains, Eckert’s experience and the contacts he made at IBM through Wood, allowed him to conceive and create the basis of the Thomas J. Watson Astronomical Computing Bureau in 1933. (Brennan 1971, 3-10)

Brennan talks both about the applications in astronomy that Eckert pioneered, and Eckert’s technical innovations with machines at the Bureau. Next Brennan touches on Eckert’s work at the Nautical Almanac Office during the Second World War, where Eckert automated operations using IBM punched card machines. At the same time, various government institutions across the country, inspired in part by Eckert’s work, set up punched card installations. (Brennann 1971, 10-12)

Brennan describes Eckert’s role in founding the Watson Scientific Computing Laboratory, hiring its staff and determining the atmosphere of free inquiry. Brennan then talks about Eckert’s role in designing the SSEC in consultation with Watson Lab staff and IBM engineers. Brennan touches on the details of such things as the preparation of the lunar ephemeris problem for the SSEC and the development of the automatic star photograph measuring engine. (Brennan 1971, 13-16, 19, 21) However, after these events, Brennan focuses on Eckert’s part in hiring and organizing research at the Laboratory.

Brennan’s *History* is a relatively detailed narrative of Eckert’s career, but it focuses on his contributions to computing and IBM. It lacks any in-depth description of Eckert’s
scientific work and mostly lists the accomplishments of Eckert and other Watson lab researchers. Still it draws on first person accounts to give some flavour of the research environment and draws some connections between Eckert and other developments in computing.

Another history, that gives Eckert a relatively prominent role, is C. J. Bashe, L. R. Johnson, J. H. Palmer and E. W. Pugh’s *IBM’s Early Computers*. This mammoth text looks at IBM hardware from its inception, with Herman Hollerith’s (1860-1929) machines in 1890, to IBM’s electronic machines of the 1940s, 50s and early 60s. The book features Eckert in his role of sparking innovation in machine design, first with punched card machines in the 1930s, and then in the design of the SSEC in the 1940s. Focusing on his machine work, it does not really elaborate on his astronomical research. However, it does examine his involvement in IBM’s strategy for carrying out scientific research: first with his role in organizing the Thomas J. Watson Laboratory at Columbia from 1945 and later as his role as advisor in setting IBM’s long term research policy. (Bashe et al. 1986, 22-25, 47-59, 525-533, 544-550) This is an aspect of his career not touched on elsewhere, but where he seems to have made a serious contribution.

Histories of astronomy tend not to deal with developments in 20th century celestial mechanics outside of relativity theory. For example, Christopher Linton’s recent *From Eudoxus to Einstein*, a 500 page book detailing the history of celestial mechanics, ends its account of developments in lunar theory with the work of E. W. Brown. (Linton 2004, 412-413) Again Martin Gutzwiller provides an exception. Gutzwiller wrote a review article on the history of lunar theory from Babylonian to modern times summarizing key theoretical and mathematical developments and included a (necessarily brief) discussion of Eckert’s various contributions to lunar theory. (Gutzwiller 1998, 624, 627-628) Still a full historical evaluation of Eckert’s work in astronomy remains to be done.

Eckert stands out among scientists as an early adopter of computers. The connection between computer methods and scientific innovation has been little touched on by
accounts in the history and philosophy of science. Some histories of computing do look into such issues, for example, Herman Goldstine’s *The Computer from Pascal to von Neumann* contains a chapter on the use of computers in meteorology. This short chapter summarizes how computer pioneer John von Neumann (1903-1957) organized a group of meteorologists who developed equations that could only be solved by computer and that allowed, for the first time, accurate weather forecasts. (Goldstine 1993, 300-305) There have also been some more extended treatments of the question.

Frederik Nebeker’s *Calculating the Weather* details developments in the science of meteorology in the 20th century. He focuses a great deal on numerical forecasting techniques and especially on the introduction of the electronic computer after World War II. He deals with other factors affecting developments in meteorology, like the surge of demand for forecasting during the two world wars, and developments in theory. However, he emphasizes computers as transforming what is possible and setting a trajectory of improved forecasting in meteorology. (Nebeker 1995, 74-76, 127-132, 143-145) Eckert’s celestial mechanics contrasts with these sorts of development in that predictive numerical astronomy was a robust and successful enterprise before the introduction of electronic computers, but the need to adapt theories to computer use and other themes are found in common.

Peter Galison’s historical and social analysis of Monte Carlo simulations (simulations that use “random” elements in their development) in his book *Image and Logic* is an in depth look at the development and use of such simulations in science, especially in nuclear physics. Galison deals with the origins of the technique, disputes about and problems with concepts such as randomness, and the characterization of such simulations as “experiments.” His greatest emphasis is on the way that the techniques of Monte Carlo simulation created a meeting point for widely disparate disciplines and also for experimental and theoretical work within disciplines. (Galison 1997, 689-780) This theme of the interdisciplinary nature of computer techniques can be seen in Eckert’s work,
especially in encouraging others to use computers.

Computer simulation, more broadly, is a subject that has been picked up by several philosophers of science. Like Galison, some philosophers attempt to discuss the idea of computer simulations as experiments, (Winsberg 2003) while others explore the extent to which computer techniques are novel and how they relate to traditional issues of how to understand approximation and idealization. (Laymon 1990) Paul Humphreys’ 2004 book, Extending Ourselves, represents perhaps the most extensive examination in this vein. Part of the book’s aim is a response to a certain kind of empiricist skepticism. However, he also spends a great deal of time developing ideas about what features of computation, especially what features of simulation, are novel to computers or illustrative of underappreciated aspects of science. One of his key concepts is the computational template. Humphreys identifies a computational template as the way theory and evidence is used to produce a set of calculations that allow predictions to be made and theory to be modified in a systematic way. In a chapter on computer simulations, Humphreys tries to define what makes simulations different from other computer techniques. He suggests that simulations should be defined as showing calculations of the time evolution of a system outputted as a time evolution itself. He suggests the useful insight, engendered by computer simulations, comes, in part, from taking advantage of human capacities such as visual analysis. (Humphreys 2004, 9-12, 60-67, 105-114) Although often focusing on the novelty of the role of the computer, these works suggest that computer techniques are often simply traditional techniques writ large. This work suggests the study of computer techniques reveal important, but often neglected, aspects of how theories are turned into models and predictions and are then tested against empirical evidence.

One other particular case of the application of computers to the sciences, that has received some attention by scholars of science studies, is computer proofs in mathematics. The most notable work on this subject is sociologist of science, Donald MacKenzie’s (1950-) Mechanizing Proof. In this book, MacKenzie details the history of various exam-
Chapter 1. Introduction

ples of computer proofs, such as early AI researchers’ work, but he focuses on the field of program verification, i.e. the attempt to use formal mathematical proof to establish the reliability of computer programs. He details the origins, motivations and diversity of this movement. For example MacKenzie talks about how the National Security Agency (NSA), in the United States, became interested in program verification as a means of assuring the security of computer systems and how this security application led to modification of practice by those engaged in program verification. MacKenzie includes a chapter on Haken and Appel’s proof of the four colour theorem by computer. On this topic, MacKenzie details the distrust and dissatisfaction, evinced by many mathematicians, for the use of a computer in a mathematical proof, instead of traditional human methods and human understanding. (MacKenzie 2001, 63-70, 137-149, 155-158, 296-298)

MacKenzie’s book examines in close detail, the way computer use is implemented in various disciplines. It suggests that the degree to which subjective evaluations of reliability determine the reception of computer techniques and their use by individual practitioners.

Another sociological look at the interaction between computers and science is found in Atsushi Akera’s Calculating the Natural World. Akera’s book is an ambitious attempt to illustrate how developments in post World War II scientific computing can be understood in terms of an “ecology of knowledge,” defined as the interaction between personal motivations, institutional environment, and personal and professional connections between individuals and groups. The book consists of several case studies, each employing different techniques, in the examination of different incidents. One chapter explores the personal and professional biography of John Mauchly (1907-1980), one of the designers of the ENIAC, a forerunner of the computer. Another looks at how IBM’s Applied Science division developed a sales culture for dealing with selling machines to scientists after World War II. Wallace Eckert is mentioned briefly in this book for his role in bringing L. J. Comrie’s (1893-1950) punched card calculating methods to the United States, in particular to the Naval Almanac Office and starting scientific research at IBM. (Akera
2007, 13-16, 41, 61, 67-105, 134, 223-246) Akera’s book suggests several different ways to explore the factors at work in the use of computers in science. Above all, it emphasizes the importance that external contingencies, such as budgets and rhetoric, can play in the development of scientific research and technological development.

David Alan Grier’s (1951-) book, *When Computers Were Human*, takes a close look at the rise of human computers as an identifiable profession in science. Grier’s book begins with the concerted efforts of a trio of French astronomers to calculate the return of Halley’s comet and ends its by discussing the fate of some human computers in the post-World War II electronic computer era. Although many of Grier’s examples are from astronomy he also finds examples of organized computing in ballistics, biology and social statistics. He sketches the character of the science involved but spends far more time discussing the times, institutions and people involved. The presence of women in computing in times and places where they were shut out of other scientific work is a recurring theme of the book. Eckert appears in Grier’s account. Grier briefly summarizes Eckert’s work and describes the relations between Eckert and other computing groups and professionals during the 1930s and up until the end of the Second World War. (Grier 2005, 192-195, 241-243) In addition to chronicling the interaction between computing and astronomy and other sciences, Grier’s book helps show the motivations, personal and professional, of those dedicated to computing.

Perhaps the most ambitious attempt at an analysis of the issue of the computer’s role in science is by scientist Donald S. Robertson. In his book, *Phase Changes*, he suggests that the computer revolutionized science in the past, just as the invention of scientific instruments, like the telescope, did. Further, the computer will continue to revolutionize science in the future. Working with ideas from information theory, he attempts to gauge the impact of computers, in terms of the orders of magnitude of new information they provide access to, or the ability to manipulate. His book is a whirlwind tour of developments involving computers in astronomy, physics, mathematics, biology
and geology. (Robertson 2003) The book provides an interesting sample of computer developments in science, but its history and analysis, while enthusiastic, is somewhat limited in depth.

All these works suggest possible ways in which to study Eckert’s work. Computer histories give Eckert a context, both as a pioneer and an early adopter of computer methods. Previous investigations of the relationship between computers and scientific practice, suggest frameworks to understand his work by offering categories of computer techniques and questions about the use of those techniques. Questions such as how colleagues responded to the techniques he introduced, what questions of reliability were raised by the new techniques and whether his use of computers changed relations between disciplines. The goal of this thesis is to explain exactly what Eckert’s work with computers entailed and to gauge its significance and implications.

This thesis proceeds, in Chapter 2, by examining the methods of celestial mechanics (positional astronomy) with emphasis on the role of computation both human and mechanical in this work. The long history of computation in astronomy both demonstrates the continuity of Eckert’s projects with earlier efforts and also the changes that his use of new technology brought about. The third chapter details Eckert’s early work and life up until the end of World War II. During this period, Eckert adapted the latest punched card accounting machines to the service of science, a precursor to his later computer work. Chapter 4 examines Eckert’s role, as an employee of IBM, in promoting the scientific use of computers and the development of new computing technology. It was at IBM that Eckert began using the first true electronic computers and began to expand the boundaries of celestial mechanics. Chapter 5 examines the two projects Eckert organized on the SSEC, the calculations of the position of the Moon and the numerical integration of the outer planets. The SSEC is the bridge between Eckert’s pre-computer work and his post-computer work and so is a dramatic illustration of the impact of electronic computers on his work. Chapter 6 examines Eckert’s later work, with various
IBM machines in the 1950s and 1960s, improving lunar theory. No longer a pioneer of computer hardware, instead, Eckert and his coworkers developed programs to make best use of standard technology. Chapter 7 sums up the results of this scientific biography of Eckert and considers the broader implications of this study.

A glossary of terms appears as an appendix to this study. The glossary includes various technical terms from celestial mechanics and computing. Given the esoteric nature of celestial mechanics with its plethora of technical terms and the obscurity into which many historical computer developments have fallen, it is hoped this glossary will help the reader find his bearings while reading this work. The glossary includes the definition of various acronyms used in this thesis.
Chapter 2

The Methods of Celestial Mechanics

Eckert’s work in computation springs directly from the problems and methods used in numerical predictive astronomy, variously called celestial mechanics, positional astronomy and physical astronomy. Therefore, I will begin with a survey of methods in celestial mechanics. The survey will trace the continuity between Eckert’s work and earlier trends and also highlight the novelty of his work.

2.1 Numerical Methods in Astronomy before Copernicus

As previously mentioned the numerical study of the motions of heavenly bodies goes back to the inhabitants of Mesopotamia in the last five centuries B.C.E.. The scholars of this era, writing on clay tablets in Babylonic cuneiform, produced tables predicting the positions of celestial bodies via arithmetic calculations. They carried out computation with the aid of their positional sexagesimal number system and their tables included the intermediate steps in their calculation. They were also aided by an older tradition in observational astronomy and records of eclipses and other events. Some Babylonian knowledge and techniques were transmitted to the Greeks in the Hellenistic period. (Lin-
 Whereas the Babylonian techniques were entirely arithmetical, the Greeks developed geometric models to explain the motion of the Moon, Sun and planets. At first the geometric models, such as the concentric spheres of Eudoxus (c.400 BCE-c.350 BCE), may have been qualitative reproductions of heavenly motion rather than providing quantitative predictions. Most of the later models involved positing the bodies to move according to circle on circle constructions (epicycle on deferent) at uniform rates with a fixed Earth at or near the centre of the system (geocentric models). (Linton 2004, 31)

The high point of Greek astronomy was the work of Ptolemy in the second century of the C.E.. He developed a complete set of models, for the Sun, Moon and planets, capable of making predictions with some accuracy, most notably the prediction of solar eclipses. Most of this was detailed in his work *The Almagest*. He built his geometric models on the earlier astronomical work of Apollonius of Perga (c.260BCE-c.190BCE) and Hipparchus (c.190 BCE-c.120 BCE) and the geometry of Menelaus (c.70-c.130). Ptolemy developed a complex geometric model for each of the visible planets, the Sun and the Moon. He was able to reduce the calculation of the longitudes of the heavenly bodies to an analogous set of operations. The motion was broken into three parts, the mean motion of the planet, the first anomaly and the second anomaly. The second anomaly depended on two independent factors making it complex to calculate. Each of the three elements of the longitude were calculated from tables that Ptolemy devised. Ptolemy also included a table of chords for performing trigonometric calculations. A chord is a straight line cutting a circle at two points. The chord can also be defined in terms of the angle between the two points measured at the centre of the circle. The tables give the length of the chord for a given angle, thus the values, in Ptolemy’s table, are equal to twice the sine times the radius of the circle (60 units for Ptolemy). Although Ptolemy was not the first to use tables, his work is the earliest example extant of their use in complex calculations. (Pedersen 1993, 76-89)
One important geometric construction, pioneered by Ptolemy, was the equant. In this construction, a planet is understood to move on a circular path around a centre (the eccentric) other than the Earth, with a constant angular motion around a third point, opposite the eccentric point from Earth. This use of non-uniform circular motion would prove controversial. (Neugebauer 1962, 193-205)

Greek astronomical techniques would be transmitted to the Sanskrit scholars of the Indian subcontinent and Arabic scholars. Arabic scholarship, in particular, would maintain and build upon the Ptolemaic tradition of astronomy and trigonometry. For example, the introduction of functions equivalent to the modern trigonometric functions of sine and cosine were respectively developed by Indian and Arabic scholars. (Linton 2004, 87, 91-93) However, no dramatic breakthroughs were evident.

One development in methods of calculation that deserves mentioned is *prosthaphaeresis*, developed by 10th century C.E. Arabic astronomer and mathematician Ibn Yûnus (c.940-1009). He pointed out that the recently discovered relation:

\[ 2 \cos(x) \cos(y) = \cos(x + y) + \cos(x - y) \]

would allow multiplication to be carried out by performing an addition, a subtraction, two table look ups and a division by two. This method was *prosthaphaeresis*, a substitute for long multiplication by hand. Such processes were far easier than the multiplication of large numbers. Although the use of this technique was never particularly widespread, it did lead to the development of tables of trigonometric functions to many more digits of precision. (Linton 2004, 96) This limited success suggests the need for means to ease the burden of calculation at this time.

Several methods of mechanical calculation were also used in ancient and medieval astronomy. One of the most ancient of such devices is the intriguingly complex geared Antikytheria Device, discovered a hundred years ago, but still yielding new results for historians under analysis. (Freeth *et al.*, 2006, 587) More long-lived was a tradition of analog calculators, beginning with Hellenistic and Roman era celestial globes and
stereographic projection of the surface of globes onto planes. Arab astronomers would perfect these techniques to produce the iconic form of the plane astrolabe, a device useful for calculating things such as rising and setting times of stars. They are called analog because they make use of a one-to-one analogy between the continuous quantity to be found in the solution (time) and some property of the device (a continuous curve etched onto the astrolabe). (Bromley 1990, 165)

Other devices were developed to compute things like the time of planetary conjunctions. (Goldstine 1993, 5) Europeans widely adopted the astrolabe beginning in about the 10th century C.E.. Europeans also developed various, more complex, analog calculating mechanisms of their own (called equatorium, or volvella), capable of finding planetary longitude. Such devices had been developed in the Islamic and Greek world, but the European devices are notable for their prominence and diversity. Although finding the values, using such devices, was faster than other methods, they lacked the precision to actually replace tables and long arithmetic calculation. (Pedersen 1993, 219-221, 228-231)

2.2 Copernicus to Newton

With the sixteenth century, the centre of astronomical developments moved to Europe. The revival of classical knowledge and the importation of Islamic and Indian techniques and knowledge in astronomy, set the stage for the radical conceptual shift of Nicholas Copernicus (1473-1543). Copernicus’s 1543 book On the Revolution of the Heavenly Spheres described a heliocentric system that placed the Sun at the centre of the solar system with all bodies orbiting around it. Copernicus modeled his work on Ptolemy. In fact Copernicus often designed his models to agree with the relative positions given by Ptolemy. However, Copernicus incorporated several constructions not found in Ptolemy, including some to eliminate the equant. Many of these constructions have precursors in the work of Islamic scholars, such as the 13th century Ibn al-Shāṭīr (1304-1376). (Linton
The mathematical apparatus Copernicus employed lacked significant innovation in terms of ways of calculating. However, issues around calculation played a role in Copernicus’s success. First, no recorded astronomer, since Ptolemy, had calculated the full set of parameters required for the geometric models of the planets, a task requiring thousands of calculation. The implication is that the labour of calculation played a role in the longevity of Ptolemy’s models. Copernicus’s model was not intrinsically more accurate than Ptolemy’s, often employing equivalent geometric constructions. However Copernicus’s work achieved greater accuracy in many, but not all, aspects. This was due to more, and better, observations being included in Copernicus’s calculations and the recalculation of parameters that had changed since Ptolemy’s day. (Linton 2004, 119, 122, 144; Kuhn 1957, 187-188)

Another computational issue, related to Copernicus’s work, was the need for tables to allow easy calculation of astronomical predictions. Such tables were based on more general astronomical works like the *Almagest*. The standard reference at the time of Copernicus’s work were the 13th century Alfonsine Tables. Erasmus Reinhold (1511-1553) would publish a set of tables, called the Prutenic or Prussian tables, that displaced the other tables then in use. The Prutenic tables were based on Copernicus’s work. Copernicus’s work became a common tool of predictive astronomy, albeit indirectly, due to the popularity of the Prutenic tables. This was despite skepticism and opposition to the literal truth of Copernicus’s theory, for example Reinhold himself did not believe in the Heliocentric system, (Jarrell 1989, 26-27; Kuhn 1957, 187-188; Linton 2004, 119, 122)

Tycho Brahe (1546-1601) is best remembered for his observational work, famously achieving an accuracy of about 1 minute of arc (1°) in many of his measurements. He is also known for his Tychonic system that left the Earth static, with the Sun and Moon orbiting it, and all the other planets orbiting the Sun. Not known for technical innovations in predictive astronomy, Brahe’s goal was a new system of astronomy based on his new
observations. The mathematical analysis and volume of arithmetic, required by Brahe’s plans, was sufficiently great that he hoped to use the method of *prosthaphaeresis* to speed calculation. (Kuhn 1957, 202; Thoren 1989, 9-14, 19-21; Linton 2004, 158-159) Brahe also discovered new phenomena requiring explanation.

A good example of this is the discovery by Brahe of what is called the variation of the Moon’s motion. He observed that the existing models of the Moon’s motion were in error because of an unaccounted for increase in the Moon’s motion at opposition and conjunction (the syzergies) with the Sun and reduction at half moons (quadrature). This showed up as a difference of 40′ of longitude between the measured position of the Moon and that obtained from theory at the relevant times. Brahe modeled this by adding another circle to Copernicus’s lunar theory. Brahe, and his assistant Longomantanus, further modified the lunar theory to take into account other deviations in apparent size, longitude and latitude not accounted for by earlier theories. (Thoren 1989, 16-19; Gutzwiller 1998, 602) The variation in particular would play an important role in some lunar theories.

The most famous outcome of Brahe’s long observational project was the new ellipse based celestial mechanics of Johannes Kepler (1571-1630). Kepler’s work has become famous as a tedious fitting of theory to data involving massive amounts of calculation. Kepler, a committed Copernican, began his work by revising Copernicus’s heliocentric theory to remove certain inconsistencies and achieve better fit with observation by, for example, putting the actual position of the Sun, rather than the mean position of the Sun, at the centre of all the orbits. (Kuhn 1957, 209-211)

Kepler began his work with the motion of Mars. Kepler concluded, after various unsuccessful attempts, that the methods of epicyclic astronomy including the equant were inadequate to fit Brahe’s accurate data. This was after 70 trials using an iterative method that required long calculations. Kepler drew his inspiration for the law from the view that the Sun provided motive power to the planets and from the fact that Ptolemy’s equants (and Copernicus’s replacements) produced faster motions when the
planets approached the Sun and slower motion when the planet moved away. From his speed law, he derived a simpler approximation that the line between planet and Sun sweeps out equal areas in equal times, which proved so effective that he finally adopted it instead. (Kuhn 1957, 209-216; Gingrich 1989, 58-64) This is popularly known as Kepler’s second law.

Having adopted this speed law, he now had to determine the shape of the orbit. After trying various shapes, he settled on an ellipse, with the Sun in one of the focuses, as the shape of Mars’s orbit and by inference, all planetary orbits. The elliptical form of planetary orbits is known as Kepler’s first law. He first published these results in his 1609 work *Astronomia Nova*. (Gingrich 1989, 64-69)

Later works refined and recapitulated these theories. In addition to the two basic laws of planetary motion, Kepler found, through calculation, various other mathematical relations in his planetary theory which he felt had special significance. Only one of these relations had significant impact on subsequent developments; Kepler’s third law, that the ratio of the periods of two planets’ orbits is as the 3/2 power of the ratio of the planets’ mean distances from the Sun. This and other harmonies were given, backed by calculations, in Kepler’s 1619 book *Harmonices Mundi*. (Kepler 1952, 1020; Kuhn 1957, 216-217)

Determining planetary positions from Kepler’s laws was a complex task. Specifically it required the solution of the relation now rendered as Kepler’s equation:

\[ E = M - e \sin E \]

where M is the mean angular motion, e is the orbital eccentricity and E is an angle related to M by Kepler’s second law. The eccentricity is a measure of the deviation of the ellipse from circularity, with \( e = 0 \) indicating a circle and \( e = 1 \) indicating a parabola. Kepler found the only way to solve this equation was a tedious iterative method. Kepler’s meth-

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1 *The New Astronomy*
2 *The Harmony of the World*
ods were so difficult and novel that it was only with the publication of a complete set of tables of calculation for his orbits, the Rudolphine Tables of 1627, that the accuracy of his work and even how it should be applied became clear to his contemporaries. As with earlier tables these eased computation compared with working with basic theory. One aid he provided was values for M in Kepler’s equation at regular values of E. (Gingrich 1989, 75-78) Even so, others found Kepler’s speed formula difficult to calculate and inelegant, leading to various approximations by other astronomers, usually relying instead on epicyclic geometric constructions. (Wilson 1989a, 172-184) Again calculation influences how astronomical theory is received and used.

Another innovation of the *Rudolphine Tables* was the inclusion of logarithm tables. Kepler had learned of logarithms from John Napier’s (1550-1617) 1614 *Mirifici logarithmorum canonis descripto*. Apparently Napier was interested in aiding in calculations in spherical trigonometry applicable to astronomy and sent some of his preliminary results to Brahe. (Kline 1972, 256)

Starting in 1594, Napier developed his scheme, not in terms of exponents and bases, but in terms of the ratios between two trajectories, one with a constant velocity, the other with a velocity proportional to its position and, therefore, given by an exponential function. The function Napier first chose for his logarithm was inverse to the modern scheme, the log of a small number was large and the log of a large number small or negative. This was due to Napier’s intention of using them in trigonometry where all the numbers to be computed upon would be between zero and a large numerical coefficient, use of decimal fractions between 0 and 1 for trigonometric functions were not yet popular. (Gibson 1914, 8-11)

Logarithms soon took most of their modern form when Henry Briggs (1561-1630), Geometer at Gresham College, consulted with Napier about how to modify the scheme in 1615. Briggs’s logarithms were equivalent to modern base 10 logarithms except for

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3 *Description of the Marvelous Canon of Logarithms*
the decimal point. In Briggs’s scheme the log of 10 would be $10^{15}$ instead of 1.

Briggs produced *Arithmetica Logarithmica* in 1624 a set of tables of logarithms for all numbers 1 to 20,000 and 90,000 to 100,000 all to fourteen digits of precision. Briggs’ tables became the basis of most subsequent tables. (Gibson 1914, 8, 11-13; Kline 1972, 257-258)

Kepler had discovered and begun making use of Napier’s work by 1617 and so did not use Briggs’s tables. Instead Kepler calculated his own set of logarithms, basically inline with Napier’s original scheme except for the numerical coefficients. (Gingrich 1989, 75-77) The quick and sustained success of logarithms, especially in astronomy, suggests both their utility in aiding calculation and the felt need for such aid.

Kepler’s Rudolphine Tables achieved unprecedented accuracy. Modern estimates suggest they are thirty times more accurate than any predecessor. The prediction, based on Kepler’s work, of the 1631 transit of Mercury demonstrated to his contemporaries the accuracy of his work, one year after his death. Pierre Gassendi (1592-1655) and other astronomers observed the transit and found Kepler’s tables to be accurate to within 6 hours of time or 15 minutes of arc, compared to the almost 5 degrees suggested by other tables. (Wilson 1989a, 164; Gingrich 1989, 77)

Kepler’s lunar theory was less successful than his elliptical orbits for the planets. Published in 1620 and 1621, Kepler’s lunar theory involved ellipses and attempted to take into account interactions between terrestrial and lunar influences suggested by Kepler’s physics and the complex motions displayed by the Moon. Beginning in 1638 Jeremiah Horrocks (1618-1641) worked out an improved geometric model where the Moon moved on an ellipse whose centre moved on a circle. Due to Horrocks’s death in 1641 this work was not published until 1672. This model would not be improved upon for decades and would provide clues for 18th century astronomers about possible solutions. (Wilson 1989a, 194-201; Gingrich 1989, 73-77)

The 17th century saw the invention of the telescope and this had important implica-
tions for astronomy. Starting in 1609, Galileo Galilei (1564-1642) observed by telescope many new phenomena including the discovery of four of Jupiter’s moons. These were new bodies and they needed new theories to predict their motions. Galileo spent considerable time tracking their motions in order to derive their orbital periods. Kepler showed that the moons of Jupiter obeyed his third law. The telescope also led to the possibility of significantly increasing the accuracy of astronomical measurement. This would allow and require even more exact astronomical theories to be put forward and calculated. In fact, more accurate measurement awaited the invention of the micrometer in the 1650s by Christaan Huygens (1629-1695) and the explanation of stellar aberration in 1729. (van Helden 1989a, 81-92; van Helden 1989b, 113-115; Wilson 1989a, 205)

As the 17th century neared its end, Kepler’s planetary theory was vindicated and superseded by that of Isaac Newton (1643-1727). Newton formulated, in an axiomatic and geometrical style, his laws of motion and his theory of universal gravitation. He showed that Kepler’s orbits imply a central attractive force varying as the inverse square of the distance. He also showed that only an inverse square force of attraction could account for Kepler’s third law. Newton’s theory of universal and mutual attraction between every bit of matter not only explained Kepler’s laws but other phenomena such as how the Moon produces the tides. Most subsequent developments in celestial mechanics until (and even after) Albert Einstein’s (1879-1955) development of general relativity in 1916 are derived in some way from the laws Newton laid out in his 1687 masterwork Philosophi Naturalis Principia Mathematica. (Wilson 1989b, 258-273)

Newton’s work showed that in principle, Kepler’s work was only an approximation, but Newton failed to significantly improve on Kepler’s methods’ predictive power. Newton’s lunar theory remained an *ad hoc* construction, building on the work of Horrocks, yet Newton managed to derive valid justifications for the values of many parameters in this lunar theory. For example the motion of the nodes of the Moon, he calculated, via

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5 *Mathematical Principles of Natural Philosophy*
geometric arguments, the mean annual motion of the nodes of the Moon’s orbit to within 3 minutes of the value observed, in his day. The nodes are the point at which the orbit of a body passes through the plane of the ecliptic, the plane of the Earth’s orbit around the Sun. Still some calculated values remained at odds with the observed values. In particular there was a rather large discrepancy with the change in the angle of apogee between orbits of the Moon. By considering a Keplerian elliptical orbit, perturbed by an outside force, Newton was able to derive a change in the angle of apogee in the Principia however it was only half the measured value.⁶ (Newton 1952, 100, 313; Wilson 1989b, 262-268) Later researchers would attempt to derive fully accurate trajectories for the Moon and other bodies using only Newton’s theory of gravity.

The first edition of the Principia included the derivation of a general method for finding the orbits of comets, a novel accomplishment that exceeded previous approaches. By assuming a parabolic shape for the orbit of the comet, Newton set out a geometric procedure for determining a comet’s orbit from three observations. To derive actual numerical predictions, he made use of both arithmetic and graphical constructions using a straight edge and compass. He gave extensive examples of how he had applied the method and derived predictions accurate to within 4 minutes of arc in longitude and latitude. Edmund Halley (1656-1742) used a modified version of Newton’s method in his own researches into comets, published in 1705. Halley eschewed the use of graphs and instead used arithmetic calculation for his orbits. Although not particularly precise or easy to use, the method stands as the first general method for finding the trajectory of celestial bodies. (Newton 1952, 333-368; Grier 2005, 13-15) The number of such calculations required would increase with each new body found and with the improved telescopic observations.

The key to making Newton’s gravitational theory an effective means of generating

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⁶Examination of Newton’s manuscripts by later researchers shows a better value, but this was achieved by a fallacious calculation. (Wilson 1989b, 263; Gutzwiller 1998, 616)
more accurate predictions was the calculus, another of Newton’s inventions. Whereas Newton stood alone in pioneering a quantitative description of gravity, the methods of calculus were discovered at almost the same time by Gottfried Wilhelm Leibniz (1646-1716). Leibniz published his discoveries, allowing others to learn and build upon it, while Newton’s work remained private for years. Leibniz’s notation also proved more successful in learning the methods and deriving new theorems in calculus. This is seen in the way that developments in mechanics in Europe, where Leibniz’s methods were widely adopted, outstripped those in England, where Newton’s methods remained dominant and were taught into the 19th century. (Boyer 1949, 196-223, 235, 237)

2.3 The Eighteenth Century: The Three-Body Problem

Alexis Clairaut (1713-1765), Leonhard Euler (1707-1783) and Jean-Bapiste le Rond d’Alembert (1717-1783) were among the first people of note to work extensively on celestial mechanics using the new tools of mathematical analysis provided by Leibniz’s calculus and developments in algebra. For example, working in the 1740s and 50s, d’Alembert and Clairaut derived a differential equation for a particle moving according to a central force varying as the inverse square of the distance from the centre:

\[
\frac{d^2 u}{d\theta^2} + u = \frac{\mu}{h^2}
\]

where \( u = \frac{1}{r} \), \( h = r^2 \dot{\theta} \), \( r \) is the distance from the centre and \( \theta \) is the angular coordinate of the body (\( h \) is constant by Kepler’s second law). In the 1730s, Euler’s work developing systematic solutions to differential equations, allowed the general equation for a two body problem to be given in the form of:

\[
r = \frac{\ell}{1 + e \cos(\theta - \theta_0)}
\]
where $\ell = h^2/\mu$ and $e = \ell A$, $A$ is a constant of integration. This is the equation for a conic section. This showed that the inverse square law implies elliptical orbits rather than the converse (which Newton had shown). This derivation was not the first proof of this, but suggests the change in methods of the 18th century, which replaced the geometrical methods of earlier times. (Linton 2004, 293-295)

Advances were also made in refining the idealizations used in celestial mechanics. In 1743, Clairaut published his mature study on the shape of the Earth and its implications for gravitation. In a book that summarized earlier work, Clairaut derived the equation for gravitational acceleration on the surface of an ellipsoid as it varied with latitude. This equation became known as ‘Clairaut’s formula’. Others, such as Daniel Bernoulli and d’Alembert, also contributed to research on these issues. (Greenberg 1995, 406, 426-427, 570)

While the two body problem was assimilated into the new analytical mechanics in a straightforward way by Euler and other 18th century researchers, the three body problem proved less tractable. One important step, in finding ways to deal with the problem, was Euler’s derivation of trigonometric series for some of the relevant functions. (Wilson 1995a, 99; Linton 2004, 297-298)

Despite this sort of progress, Clairaut, d’Alembert and Euler, like Newton, could not derive the observed motion from Newton’s laws. As a result, in 1747, Clairaut announced that Newton’s law had to be modified by the inclusion of an attraction term varying as $1/r^4$. At first d’Alembert and Euler were also persuaded of the need to add additional attraction terms. However, the Comte de Buffon criticized this suggestion on the metaphysical grounds that it violated the simplicity that a fundamental law should have. (Gutzwiller 1998, 607; Linton 2004, 299)

This controversy led Clairaut to return to his calculations and take his approximations

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7d’Alembert suggested rather than modifying gravity that some other force was also at work. (Linton 2004, 299)
Chapter 2. The Methods of Celestial Mechanics

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further. He had derived an expression equivalent to:

\[
\frac{k}{r} = 1 - e \cos q \theta + A \cos 2n \theta + B \cos (2n - q) \theta + C \cos (2n + q) \theta
\]

where \( n = \frac{(n_M - n_S)}{n_M} \), \( n_M \) and \( N_S \) are respectively the mean motion of the Moon and Sun, \( k, e, q, A, B \) and \( C \) constants to be determined. At first Clairaut had only considered the first term in this series and derived the erroneous value for the motion of lunar apogee. Eventually he undertook the labour to derive the values for the other terms in the series. Upon completing this, he found that most of the observed value of the motion of apogee was correctly predicted. The terms, previously neglected in the series, were equivalent to the contribution from the force on the Moon, tangential to its orbit, that had also been neglected in Newton’s approximation. He announced this in 1749 and spurred further analysis by d’Alembert and Euler. He published his lunar theory in 1752 and a set of tables for calculating the Moon’s position in 1754. (Linton 2004, 302; Brown 1960, 238-239)

D’Alembert criticized Clairaut’s work because of its use of numerical values derived from observation rather than proceeding from analysis of the equations. D’Alembert worked out the improved value for lunar apogee in a set of equations worked out purely algebraically. D’Alembert deposited a lunar theory with the French Academy in 1751 and published his lunar theory in two volumes in 1754 with tables following in 1756. Part of d’Alembert’s reason for calculating all this was to establish the originality of his work against Clairaut. (Brown 1960, 239; Briggs 1971, 114)

The differences between the approaches of d’Alembert and Clairaut, illustrate an important distinction between two different ways of deriving orbits in celestial mechanics. In order to obtain numerical values for the trajectory of a celestial body, certain numerical properties must be substituted into the equations of motion for a body. For example, all the positions, masses and velocities of the bodies, at a given time \( t \), would be sufficient to find all past and future values, but other properties can be used. However, the form of the solution can depend, in complex ways, on the relationship between those properties and so
on their relative values. Therefore, employing measured values of the properties can avoid the labourious calculations and quickly establish certain algebraic relations in what is called a numerical solution. On the other hand, deriving the solutions with all constants in algebraic form, so-called literal solutions, can be easily updated in light of more accurate measurement of numerical constants thereby making comparison with different theories (with different values for the numerical constants) easier. A similar distinction can be see in other branches of physics between ab initio methods, where values are obtained from first principles, as opposed to empirical or semi-empirical approaches, that use experimental values as a starting point to derive further properties rather than tortuous or impossible theoretical derivations of the intermediate terms.

Euler also managed a derivation of these results. In 1753, he published a book on lunar theory that included two different approaches. His primary approach solved the differential equations algebraically for most of the constants, but numerically for \( m \), the ratio of the Sun’s and Moon’s mean motions. In the appendix to the work, he also suggested using a method equivalent to the variation of arbitrary constants\(^8\). This method would become quite widely used in later celestial mechanics. Under this method, the motion of the motion of the Moon (or other body) is considered to be an ellipse, however the constants that define the ellipse are taken as variable. The six constants usually used to define the Kepler ellipse, forming the dependent variables for this approach, are as follows: the semi-major axis \( a \), the eccentricity \( e \), the inclination \( i \), the longitude of ascending node \( \Omega \), the longitude of perihelion \( \bar{\omega} \) and the time of perihelion passage \( \tau \).\(^9\) The major axis is the larger axis of symmetry of the ellipse and the semi-major axis is half that distance. The inclination is angle between the ellipse and the ecliptic. (Brown 1960, 239-240)

Although Euler obtained the same results as Clairaut and d’Alembert, he was un-

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\(^8\) Also, called the variation of orbital parameters
\(^9\) In Euler’s time the value for aphelion was used rather than perihelion.
satisfied with both approaches. In particular, he was unable to explain the observed secular acceleration of the Moon. As a result, he suggested that resistance from some interplanetary medium (the ether) leads the Moon to fall toward the Sun, increasing its distance from the Earth and so its relative velocity. (Linton 2004, 314)

As suggested, these researches had not completely explained the motion of lunar apogee. Whereas Newton had only explained half of the motion, Clairaut and other researchers explained 85% of the motion. This was still enough to create a consensus that Newton’s theory could explain the lunar motion. As noted, Clairaut and d’Alembert produced tables for the prediction of lunar motion, based on their new theories, and Euler had previously published lunar tables, but none were sufficiently accurate for use in navigation. While all were more accurate than Newton’s effort, they still contained errors as great as 5’ of arc. This changed in 1753 when Professor Tobias Mayer (1723-1762) of the University of Goettingen produced a new set of tables that achieved an accuracy of about 1’. Mayer’s work was based on Euler’s lunar theory, but extended the theory and augmented it by careful use of observations. Mayer’s work made shipboard observation of the Moon a viable method to determine longitude at sea. (Linton 2004, 304; Gutzwiller 1998, 610)

One significant innovation of Mayer’s work was the combination of observations to derive the parameters of lunar theory. The work occurs in a 1750 memoir on the librations of the Moon. The libration is the deviation of the Moon’s rotation that leads its orientation with respect to the Earth, to change slightly. The parameter, which Mayer needed to determine, was the inclination of the Moon’s equator relative to the plane of the ecliptic. Twenty-seven observations of a crater on the surface were used to generate twenty-seven equations of condition, each with three unknowns. These equations were grouped into three groups of nine and the nine equations in each group added together. The three summed equations were used to solve for three unknowns, one of which was the inclination, essentially finding the value of the unknown parameter as a parameter
of best fit to the observations. This was a more complex averaging of observations than found in earlier practice. Earlier, in a 1749 memoir, Euler had failed to devise a way to combine observations of Saturn when considering calculations of the mutual interactions between Saturn and Jupiter. (Stigler 1986, 17-31) Mayer’s work marks an important innovation in developing systematic and standardized techniques for reducing observations into general parameters of solutions.

In 1773, Euler published a second work on lunar theory. Here, he put forward a very different method for finding the Moon’s motion. The main innovation of this method was the use of rectangular coordinates, rotating with respect to the Earth in the plane of the ecliptic with the mean motion of the Moon. Euler’s previous solution had used cylindrical coordinates and those of others used spherical polar coordinates. He also introduced some significant innovations in solving the differential equations. (Brown 1960, 340-341) However, he did not achieve any significant increase in accuracy and failed to explain the secular acceleration of the Moon in terms of gravitational effects. Slightly earlier, in 1767, he managed to find exact solutions to the restricted three body problem, where the mass of the third body is so small that it does not perturb the other two bodies. (Linton 2004, 323)

One question raised by the use of infinite series solutions in the lunar problem was whether the series converged. A convergent series approaches some definite value, an asymptote, as more terms are included. Technically, convergence requires that the asymptote be the limit as the number of terms goes to infinity. For practical use, a series must achieve a good approximation with only a small finite number of terms actually employed. In fact a series could in theory diverge at the infinite limit but still provide a good approximation with a finite number of terms. Later in his career Euler would worry about whether the series in the lunar problem would converge. More generally, d’Alembert devised a test for convergence of series still known as d’Alembert’s theorem. (Briggs 1971, 116; Linton 2004, 297) The convergence of series, both in principle and in
practice, is a recurrent issue in celestial mechanics, but was little studied at this time.

Another victory for Newtonian theory, brought about by Clairaut’s work, was a determination of the return date of Halley’s comet. Halley had left an uncertainty about its return, saying it would return either late in 1758 or early 1759. Clairaut worked on calculating the date of perihelion for the comet, beginning in 1757. The calculations were so intensive that he required the assistance of two others, Joseph Jérôme de Lalande (1732-1813) and Madame Nicole-Reine Lepaute (1723-1788). Even with their aid he was unable to complete all his analysis before the return. This forced him to give a preliminary prediction in November of 1758 of April 15th 1759 and qualification that left a margin of error of about a month on either side. The comet was sighted Christmas day of 1758 and the actual date of perihelion was found to be March 13th. The end result of his first analysis, completed after the comet had passed, gave April 4th as the date and a further analysis, finished in 1762, gave a calculated date of perihelion of March 31st. His inability to give a more accurate date led to criticism from d’Alembert and others. However, this prediction of the return of Halley’s comet was still seen as a vindication of Newtonian theory by many. (Linton 2004, 304-306; Itard 1971a, 283; Grier 2005, 23)

Clairaut’s calculation is of note for another reason. In order to account for the effects of Saturn and Jupiter he was forced to use techniques involving mechanical quadrature, also called numerical quadrature or what we would now call numerical integration, to account for the departures from elliptical motion. This was one of the first large scale employments of such a numerical technique. (Linton 2004, 305)

Geometrically, quadrature involves inscribing a polygon in a curve so that an approximation of the area under the curve (or length of the curve) can be derived. Arithmetically, quadrature is achieved by treating some of the continuous variables as if they changed in discrete steps. This is straightforward for three or more body problems in Newtonian physics, where the acceleration due to gravity can be directly calculated for any position of a body, given the position and masses of the other bodies.
A simple form of numerical integration can be performed in the following way: Starting from an initial measured position, velocity and acceleration, the acceleration can be assumed to remain the same for some short period (the time step or interval) and a new position and velocity calculated for the next point in time on that basis. The acceleration can be recalculated for this new position and time and the process can be repeated as many times as necessary to derive an approximate trajectory. More complex formulae for extrapolating from one step to the next can be used. For example, velocity can be assumed to increase linearly over the time step. Similarly, the change in accelerations can be extrapolated from previous points in the trajectory. Such calculations are straightforward and accurate, provided the time step is small enough. However, an exceedingly large number of arithmetic calculations are required. Also, it gives only numerical values for particular points in time not equations good for all time. As a result the technique was rarely used in celestial mechanics before the 20th century. For example, Euler had suggested that such a technique could be used to integrate values in the lunar problem. One of his colleagues Anders Lexell attempted this, but ultimately achieved little. (Linton 2004, 297-298)

 Clairaut and his partners did not attempt to derive the entire orbit of Halley’s comet by numerical integration. Instead they began with an elliptical orbit and modified it at every 2′′, in light of the perturbations due to Jupiter and Saturn. These perturbations were calculated and added to the basic ellipses via numerical integration. This procedure was not used throughout the entire orbit and various shortcuts were employed when the comet was distant from the Sun, Jupiter and Saturn. The combination of an elliptical base orbit, with numerical integration, would become a standard technique in celestial mechanics known as Encke’s Method. Johann Encke (1791-1865) was a 19th century astronomer who computed the orbit of the second periodic comet discovered, now known as Encke’s comet. Although each step in Encke’s method is more complicated than the steps in direct numerical integration, the interval between calculation can be larger.
Despite the successes of Newtonian theory, problems remained that proved intractable for the astronomers and mathematicians of the mid-eighteenth century. Chief among these were the secular acceleration of the Moon and accounting for the variation in the orbits of Jupiter and Saturn due to their mutual gravitational interaction. As mentioned previously, Euler’s attempt to derive the interaction of Jupiter and Saturn failed. One aspect of the failure deserves note. He derived an equation for the difference between the average distance from the Sun to Saturn and the actual distance in terms of Jupiter’s position with the form:

$$x = A \sin m\theta + \frac{a_1 \sin p_1 \theta}{m^2 - p_1^2} + \frac{a_2 \sin p_2 \theta}{m^2 - p_2^2} ...$$

Where $A$ is an arbitrary constant, $m$, $a_i$, and $p_i$ are empirically derived constants and $\theta$ is the eccentric anomaly of Jupiter. The eccentric anomaly of Jupiter defines a unique position for Jupiter in its orbit and so a unique time. The form of the solution made it impossible to neglect the later terms in the trigonometric series, since, if $m$ and one of the terms $p_i$ were close in value, the effect of that term would be magnified greatly. This is the problem of small divisors and it remains a significant problem for the solution of differential equations using series solutions. (Linton 2004, 312-314)

The latter part of the eighteenth century saw these motions successfully accounted for in the work of two of the most famous practitioners of celestial mechanics, Pierre-Simon Laplace (1749-1827) and Joseph Lagrange (1732-1813). One of Lagrange’s earliest successes in mechanics involved questions of the Moon’s revolution, rotation and librations begun in 1762, with refinements added as late as 1780. Lagrange gave a rigorous account of why the Moon’s rotation locks one of its faces towards the Earth. Lagrange also derived theoretical values for the deviation from the locked relation. This lunar work of 1762 was of particular significance, because it was the first application of principles (in particular the principle of virtual velocities) that Lagrange would use to formulate his celebrated form of the equations of motion. These equations are a cornerstone of classical
physics and now written in the form:

$$\frac{d}{dt} \frac{\delta L}{\delta \dot{q}_\alpha} - \frac{\delta L}{\delta q_\alpha} = 0$$

Where $L$ is the Lagrangian (the function equal to the kinetic energy minus potential energy) dependent on $q_\alpha \dot{q}_\alpha$ and $\alpha = 1, \ldots, N$. $N$ is the number of degrees of freedom of the system. The $q$'s are the positions of the mutually interacting bodies. The principle of virtual velocities is invoked by imagining an infinitesimal displacement of the bodies (giving them ‘virtual’ velocities) finding the work done as a function of the kinetic energy (or *vis viva* as it was known in the 17th century) and equating this to a change in the potential energy. By the 1780s Lagrange had come to his mature formulation of these relations and in 1788 his *Traité de Mécanique Analytique* was published. (Linton 2004, 319-323; Itard 1971b, 562, 568; Fraser 1983, 226-227)

The Lagrange points were another eponymous discovery of Lagrange. In 1772, in response to a series of essay competitions, Lagrange worked on finding exact solutions to the three body problem. As a result, he discovered five theoretical trajectories that admit of exact solutions, on the assumption that the ratios of the distances between the masses remained constant. For all five solutions the position of the third body relative to two reference bodies is constant so the five points (one for each solution) became known as the Lagrange points. Until the discovery of bodies orbiting the Sun-Jupiter system near the Lagrange points in the 20th century, this discovery had little practical impact on celestial mechanics, but represents a significant advance in the mathematical treatment of the three body problem. (Linton 2004, 323-326)

In the course of studying the mutual interactions of the satellites of Jupiter, Lagrange uncovered an important limitation to the use of trigonometric series. Lagrange attempted to use the same technique of successive approximation that Clairaut had used for the lunar problem. However, in this case, Lagrange found that the method of solution created spurious secular terms. These terms grew despite the periodic or near periodic motion of the Jovian satellites. Therefore the solutions would eventually cease to be accurate
as the secular terms grew and dominated over the periodic elements. This led Lagrange to focus on using the method of variation of arbitrary constants to solve problems in celestial mechanics. (Linton 2004, 327-328)

Lagrange’s work would be the basis for the program of predictive astronomy carried out by Pierre-Simon, Marquis de Laplace. Laplace began his work in the 1770s in concert with Lagrange. They derived general equations for two planets in orbit around the sun. Laplace used this work to show that the semi-major axis has a secular term that is approximately zero. Lagrange was later able to provide a more general proof that the coefficient of the secular term for the semi-major axis was equal to zero. Apparently secular changes in the parameters of the planetary orbit, including the angles of perihelion, node and inclination, had been noted by observers since the 17th century. In 1774, Lagrange demonstrated that these changes could be explained in terms of very long period motions. This included motions of Saturn and Jupiter with a period of over fifty thousand years. (Linton 2004, 330-331) This suggested that the solar system could be shown to be stable due to the action of Newtonian forces.

Laplace worked on a variety of topics between 1775 and 1785. One important development in celestial mechanics was working out the potential function for a spheroid. Here Laplace combined efforts with Adrien-Marie Legendre (1752-1833). In fact the equation he derived for the potential function contains the function now known as the Legendre polynomials (known historically as Laplace coefficients). (Linton 2004, 338-341)

One specific noteworthy achievement of Laplace at this time was devising an analytical method for giving orbits to comets. As noted, Newton had included a geometric method in his own work, which was modified by later researchers. Laplace’s stands out as both fully analytic and practical. It begins by exploiting the fact that the acceleration of the comet due to the Sun, minus the acceleration of the Earth due to the Sun, is equal to the acceleration of the comet in geocentric coordinates. The position of the Earth is previously known, the two components of angular position of the comet relative to
the Earth needs to be determined by at least a few observations. Since the heliocentric position of the comet depends straightforwardly on its geocentric position and the heliocentric position of the Earth, the unknowns are the distance from Earth to comet, the velocity of the change in that distance, its acceleration, the angular velocity of the comet relative to the earth and its angular acceleration. The angular velocity and acceleration can be estimated from observation provided observations are closely spaced in time. The derivation of the solution must then be done numerically but can be simplified by the assumption of a parabolic trajectory. This method introduces a great risk of error, especially in calculating the angular velocity and acceleration from observations; however, the straightforward nature of the method may explain its adoption. The method remained a subject of study and modification well into the 19th century. (Linton 2004, 367-368, 371)

It was also around 1785 that Laplace was able to explain the apparent secular acceleration in Jupiter and Saturn’s orbits in Newtonian theory. First, Laplace was able to establish, based on considerations of the conservation of energy and the small masses of the planets compared with the Sun, that the expected acceleration of the two bodies due to their interactions was consistent with their observed acceleration. Laplace noted that the mean motions of Jupiter \( n \) and Saturn \( n' \) had the relationship \( 2n \approx 5n' \) implying the possibility of a resonance effect of significant power. From this insight he managed to derive an accurate description of a long period, 900 year, interaction between Saturn and Jupiter. (Linton 2004, 331-332) This marked another confirmation of Newtonian Gravity by what had at first appeared to be an anomaly.

At the same time as Laplace developed his theory for Jupiter and Saturn, he also developed his famous proof of the stability of the Solar System. This built on Lagrange’s demonstrations that the interaction of two bodies led to only periodic motions and therefore limits existed to changes in the size of the orbits in a three body system. Laplace extended this sort of analysis, augmented by assumptions about the negligible mass of
the planets relative to the Sun, the incommensurability of the periods of their orbits and
the accuracy of certain approximations. His proof also depended on the fact that all the
planets orbited in the same direction. Although, subsequent challenges have been raised
against Laplace’s proof, especially Henri Poincaré’s (1854-1912) demonstration that the
truncated series used in the proof rarely converge, it was, at the time and for a long time
after widely admired as one of Laplace’s great achievements. (Linton 2004, 349-350)
It also represents the success of 18th century celestial mechanics in explaining general
features of the universe rather than just making specific predictions.

Jean-Baptiste Joseph Delambre (1749-1822) published a set of tables in 1789 based
on Laplace’s work on Jupiter and Saturn. Laplace derived the values of the parameters
for his equations of Saturn and Jupiter by means of equations of condition. He calculated
as many as 24 equations from 24 observations. Adding and subtracting these equations,
in various ways, would derive equations to determine the corrections for the parameters.
He would then compare the equations of position with corrected parameters to further
observations. Laplace’s data fitting techniques improved on Mayer and led to a wider
dissemination and appreciation of the use of equations of condition for deriving the
parameters. Laplace would later describe Delambre’s tables as the first produced purely
from theory, because the observations were only used to find the constants of integration
of the differential equations. (Linton 2004, 345-346)

Through the 1770s and 80s Laplace also searched for an explanation of the Moon’s
secular acceleration. By 1780, he was able to rule out the deviation from sphericity
of the Earth and Moon as the culprit. The problem drove him to speculate that the
acceleration might be explained by postulating a finite propagation speed for the force
of gravity equal to seven million times the speed of light. In 1787 Laplace investigated a
very slow oscillation, hundreds of thousands of years long, in the Earth-Moon system’s
eccentricity. The motion would increase the distance between Moon and Sun thus leading
to increased acceleration of the Moon by the Earth. Laplace calculated a value of 10″
a century for this motion and, although this left 15% of the acceleration unaccounted for, it became the accepted explanation for the Moon’s secular acceleration. In the 1850s John Couch Adams (1819-1892) took the calculations of the effect to greater precision, reducing the acceleration predicted to $6''$ and reopening the question of the cause of the Moon’s secular acceleration. (Linton 2004, 333-334; Brown 1960, 243)

In 1788, as Laplace explained these anomalies for Newtonian gravity, Lagrange published the first edition of *Analytical Mechanics*, his *magnum opus*. He begins the work with the following statement:

> There are no figures in this treatise. The methods that I propose require neither constructions nor mechanical reasoning, but only algebraic operations that are bound to a regular and uniform procedure. People who like analysis will see with pleasure that mechanics has become a part of it, and they will be grateful to me for having expanded its range. (Quoted in Gutzwiller 1998, 613)

By the end of the 18th century, less than a hundred years after Newton wrote his geometrically based *Principia*, mechanics had become a subject amenable to treatment in completely algebraic and analytical terms thanks to developments started on the continent by Newton’s rival Leibniz.

In *Analytical Mechanics*, Lagrange codified and expanded upon his earlier work. He set forward a standard set of differential equations in terms of the six parameters of an elliptical orbit (semi-major axis (a), eccentricity etc.) used in the variation of parameters method. Lagrange relied for his derivation of these equations on a notation known as the Lagrange brackets. The Lagrange brackets are defined as follows:

$$(c_i, c_j) = \frac{\partial x}{\partial c_i} \frac{\partial \dot{x}}{\partial c_j} - \frac{\partial x}{\partial c_j} \frac{\partial \dot{x}}{\partial c_i} + \frac{\partial y}{\partial c_i} \frac{\partial \dot{y}}{\partial c_j} - \frac{\partial y}{\partial c_j} \frac{\partial \dot{y}}{\partial c_i} + \frac{\partial z}{\partial c_i} \frac{\partial \dot{z}}{\partial c_j} - \frac{\partial z}{\partial c_j} \frac{\partial \dot{z}}{\partial c_i}$$

Where x, y and z are the position of the body and $c_i$ is one of the six parameters of the elliptical orbit. A key property was that the partial derivative of a bracket with respect to time was zero, thus ensuring that time is not an explicit variable in the equation. Lagrange’s new equations offered a new standard and significant simplification for
practitioners of celestial mechanics. Again notation proved to be an important tool for
the mathematical physicist. At the beginning of the 19th century, Poisson would further
simplify the derivation of Lagrange’s equations with the aid of another set of notation
the Poisson brackets, a modification of the Lagrange brackets. (Linton 2004, 336-337;
Gutzwiller 1998, 614)

At the turn of the 19th century, Laplace wrote his most comprehensive works in
celestial mechanics: *System of the World* (1796) and *Celestial Mechanics* (five volumes
published between 1799 and 1825). *System of the World* summarized the discoveries of
the past century in applying Newtonian gravity to the world without the use of equations.
Despite the absence of equations the work remained highly technical and detailed. In
addition to discussing his previous work, such as the stability of the solar system, Laplace
also put forward his nebular hypothesis to explain the formation of the Solar System.
The nebular hypothesis explains the origin of the planets as arising from the collapse of
a giant cloud (nebula). Laplace’s idea was anticipated by Emmanuel Kant (1724-1804)
who published his speculation that the properties of the solar system could be explained
by his version of the nebular hypothesis in 1755. (Crowe 1994, 45-70, 175, 188; Linton
2004, 346-353)

*Celestial Mechanics* was Laplace’s tour de force in which he set out to do the sys-
tematic work summarized in the *System*. Specifically he derived accurate motions of the
planets and other celestial bodies from Newtonian theory alone. He began by reviewing,
in detail, the methods and principles of celestial mechanics. In later sections he applied in
detail his earlier developments accounting for the anomalous motions of Saturn, Jupiter
and the Moon. Laplace made use of an assistant, Alexis Bouvard (1767-1843), in making
the extensive calculations required for this work. (Morando 1995a, 143-146)

Despite using the method of variation of arbitrary constants for planetary orbits,
Laplace’s method for the Moon resembled that of Clairaut and d’Alembert. He started
from an approximate solution and found corrections. In doing so he also substituted in
several empirical constants at the beginning of his derivations. The most important of these is the ratio of the mean motion of the Sun and Moon, \( m \), which would continue to be a problem for later theories to derive. This made the derivations hard to follow and led to the theory being called “semi-algebraic.” Laplace’s theory of the Moon attained accuracy better than 30", less than half the error of previous efforts. (Gutzwiller 1998, 611-612; Brown 1960, 242-243; Linton 2004, 408) Laplace’s work set the standard for subsequent investigations of planetary motion.

2.4 New Planets, New Methods

In the late 18\textsuperscript{th} century, the telescope uncovered a new phenomenon that added to the work of celestial mechanics. In 1781 Friedrich Wilhelm [William] Herschel (1738-1822) discovered the planet that would be known as Uranus. At first it was suggested that it was a comet, but it did not conform to a parabolic orbit. This led to the realization that the object was a planet and circular orbits were used for the first approximation, until an elliptical orbit could be derived. Uncovering observations of Uranus, that predated Herschel’s observations, made the task of deriving the parameters of its orbit a great deal easier. The earliest such observation by John Flamsteed (1646-1719), the first Astronomer Royal, in 1690, was uncovered soon after the discovery. Yet major anomalies remained in Uranus’s orbit for decades. An orbit computed in 1784, based on early and later observations, gave results of acceptable accuracy at first, but by 1789 the orbit was in error by as much as 30". Another orbit, computed in 1791, that included the effects of Saturn and Jupiter based on Laplace’s work, seemed like a success, but by 1800 theory and observation diverged again. (Linton 2004, 357-358, 372-373) Other than the planetary satellites and various comets, no new bodies were found until the 19\textsuperscript{th} century and so no general method of deriving planetary orbits from minimal data existed.

The end of the 18\textsuperscript{th} century saw significant advances in the development of general
methods for deriving new orbits for celestial bodies. In 1796 Heinrich Olbers (1758-1840) used a new method, simpler than Laplace’s method, to find the orbit of a comet. He published an account of it in 1797. Olbers method would continue to be used throughout the 19th century. (Multhauf 1974, 198)

Key to Olbers’s method was taking advantage of a relationship between three positions in an orbit at three times. In vector notation (which did not exist at the time) the key relations can be represented as follows:

\[ \mathbf{r}_2 = c_1 \mathbf{r}_1 + c_3 \mathbf{r}_3 \]

where \( \mathbf{r}_i \) are the positions at three times and \( c_i \) are numerical constants. This relation follows from the fact that all the points in an orbit occur in a single plane. The two constants can be calculated from the following relations:

\[ c_1 = \frac{|\mathbf{r}_2 \times \mathbf{r}_3|}{|\mathbf{r}_1 \times \mathbf{r}_3|} \quad c_3 = \frac{|\mathbf{r}_2 \times \mathbf{r}_1|}{|\mathbf{r}_1 \times \mathbf{r}_3|} \]

The absolute value of the cross product of two position vectors is equal to twice the area of the triangle between them. Those triangles are roughly equal in area to the sector of the ellipse between the two points. By Kepler’s laws the areas of a sector between two positions in the orbit are proportional to the time interval between the positions. Therefore, the equation for the constants of is given by the equations:

\[ c_1 = \frac{t_3 - t_2}{t_3 - t_1} \quad c_2 = \frac{t_2 - t_1}{t_3 - t_1} \]

In principle, a sufficiently accurate determination of \( c_1 \) and \( c_2 \), combined with the angular values of the position determined from observations and knowledge of the Earth’s position, would allow an estimate of the remaining elements of the position. Knowing two positions, with times, in an orbit is sufficient to estimate an orbit assuming that it obeys Kepler’s laws. Therefore, an orbit can be calculated from two of the approximate positions and compared with the third approximate position. These relations were in fact first noted in 1733 by Pierre Bouguer (1698-1758). However, his suggested methods
for making the necessary approximations were insufficiently accurate and only with Olbers did this become the basis of a practical method. Olbers method involved a rapidly converging series of calculations to derive the solution. (Linton 2004, 368-369; Multhauf 1974, 198)

In 1801, soon after Olbers detailed his method, a new object was discovered orbiting the Sun by Giuseppe Piazzi (1746-1826). This body would become known as Ceres and was the first of the minor planets or asteroids to be discovered. Finding an orbit for this new planet led the mathematician Carl Friedrich Gauss (1777-1855) to devise a new method. It was required because of a paucity of data and the fact that older methods used for comets assumed a parabolic, rather than an elliptical orbit. Although, Gauss would publish an orbit for Ceres before the end of 1801, he spent eight years refining his method. He published his investigations in *Theoria Motus* in 1809. This book would become a classic of astronomical theory, still used as a reference by textbooks in celestial mechanics 150 years later. (Linton 2004, 361-362; Danby 1992, 185)

Gauss systematically dealt with all the aspects of the solution, including refining the fit of the calculated orbit to observations by the method of least squares. In 1805, Legendre had been the first to publicly propose that minimizing the square of the errors should be the measure of good fit between observations and a proposed curve and detailed it in a work on new methods in determining comet orbits. Before this the sum of the absolute value of the errors had been commonly used by the likes of Laplace. In *Motus* Gauss advocated for the least squares method and included the methods required to achieve a least squares fit and referred back to Legendre’s work. Gauss also claimed, somewhat improbably, that he had used the principle of least squares since 1795 when he was 18. (Stigler 1986, 50-61, 139-146; Gauss 1963, 269-271)

Gauss’s method was built on the same fundamental relations as Olbers’s method. However, Gauss achieved algorithms for deriving approximations that were effective at

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giving good accuracy with a paucity of data. Rather than solving for the elements themselves, intermediate values (labeled P and Q) were chosen that could be conveniently approximated from the observations and that, once determined, allowed a straightforward calculation of the elements of the orbit. The relation between P and Q and the elements was given by what is sometimes called Gauss’s equation:

\[ \alpha \sin^4 z = \sin z - \beta \]

z is the angle between the heliocentric and geocentric position vectors for the second observation, \( \alpha \) and \( \beta \) depend on P and Q. The equation has two or four roots, one of which is selected based on a straightforward criterion. Also, successive approximations of P and Q could be calculated based on the latest estimate. Gauss claimed that good accuracy could most often be achieved by the third approximation, but often by the second and even sometimes by the first. (Linton 2004, 368-370; Gauss 1963, 172, 202-203)

Within certain limitations, three observations could be sufficient to determine the orbit with considerable accuracy. The method would not work for observations too close together (where error would dominate) or too far apart (where the assumptions of the method break down). However, Gauss claimed that the method was effective on the one hand for observations of Ceres 260 days apart, separated by 62° of the sky, and on the other hand of observations of Juno 22 days apart separated by 7°. Also, at least partial data from four observations would be required for orbits in or near the same plane as that of the Earth’s orbit. (Gauss 1963, 161-163, 171-172)

With the work of people like Gauss and Laplace the basic methods of celestial mechanics had been developed and expounded. The rest of the 19th century would see these techniques refined and used to make more accurate theories and address new discoveries. However, more powerful and subtle techniques were also developed. Ceres was the first of many asteroids discovered by telescope and analyzed by Gauss’s method. (Linton 2004, 363)
2.5 Lunar Theory After Laplace

In the wake of Laplace’s epoch making work, the methods of celestial mechanics were refined and applied to more difficult problems. One of the most famous is the discovery of Neptune by inference from the perturbations it caused in the orbit of Uranus. The discovery illustrated both the power of celestial mechanics and its limitations, since both initial orbits, inferred from the perturbations of Uranus, deviated significantly from the actual orbit of Neptune. Neptune’s discovery helped put an end to the large and mysterious errors in Uranus’s orbit. (Morando 1995b, 214-222) However for the purposes of this work we will focus on developments in lunar theory.

Several attempts to improve lunar theory were undertaken over the course of the nineteenth century. Laplace had suggested that an accurate lunar theory could be achieved by analytical methods with the numerical substitution only for the arbitrary constants of integration. At his suggestion the French Academy set a prize for achieving this. It was awarded to two works in 1820, one by Frenchman Marie-Charles-Théodore de Damoiseau (1768-1846), the other by Italians Giovanni Plana (1781-1864) and Francesco Carlini (1783-1862). (Linton 2004, 408)

Damoiseau followed closely Laplace’s form of solution, attempting to fit numerical results via equations of condition. Despite the prize’s stipulation, Damoiseau substituted numerical values at the outset and used them throughout. The theory worked out the value of the coefficients to a precision of a hundredth of a second of arc. His tables, published in 1824, achieved an accuracy of 4 seconds of arc. This was a significant improvement over Laplace’s work and ensured their use for decades. (Linton 2004, 408; Brown 1960, 244)

Plana and Carlini began their work in 1811. They followed Laplace’s method but avoided his substitution of numerical values for the parameters. The series solutions in terms of the parameter \(m\), the ratio of the Sun and Moon’s mean motions, converged slowly requiring in some cases the approximation be carried out to the eighth order.
The process of refining the theory for publication took until 1832 by which time Carlini had stopped working with Plana. The publication of Plana’s *Theory of the Motion of the Moon* was in 3 volumes, it totaled over 2500 pages. Despite the voluminous size, it was slightly less accurate than Damoiseau’s theory, due to the slow convergence of the solution and errors in calculation. (Linton 2004, 408-409; Brown 1960, 244)

In 1833, famed French mathematical physicist Siméon Poisson (1781-1840) made his own contribution to lunar theory. He implemented an iterative method, involving the variation of arbitrary constants, that found successively better values for the parameters by substituting each new approximation back into the method. Poisson’s method was never carried out as a complete theory. The method becomes more involved with each iteration and too many iterations would have been required to obtain accurate results for the deviations from elliptical motion (inequalities) due to the Sun for it to be a viable method. The method was able to deal more effectively with other sources of inequalities, especially secular and long term inequalities. It had wider applicability to planetary theory. (Brown 1960, 245; Linton 2004, 409)

At about the same time, between 1830 and 1834, John Lubbock (1803-1865) published a fully literal lunar theory. The method was one of continued approximation starting from an elliptical first approximation. It differed from many earlier methods in using the time (rather than the longitude) as the independent variable. However this work was never turned into a full theory, for example it only worked out calculations to the second approximation. Philippe, Comte de Pontécoultant (1795-1874) would later pursue the same approach independently. He produced his own more complete theory (taking work out to the fifth approximation) in 1846. (Brown 1960, 12, 245)

In the middle and latter half of the nineteenth century developments in lunar theory continued. Peter Hansen (1795-1874) contributed a very accurate lunar theory, published in 1838, based on his methods for planetary theory. The method was based on the variation of arbitrary constants. The theory was numerical with all the numerical values
substituted at the start of the process. Also, the mean motion of the Moon’s perigee was obtained from observation rather than by calculation from theory. (Brown 1960, 13-2, 160-161, 188)

Hansen found a starting orbit that would allow a more efficient calculation of perturbations, as he did with his planetary theories. In this case he found a function, \( W \), which facilitated the calculations of all perturbations occurring in the plane of the Moon’s orbit. Hansen also made his method amenable to adding perturbations due to the planets’ influence. Indeed Hansen was the first to give numerical values to planetary perturbations of the Moon’s orbit. However, his calculation of the effect of Venus (the largest correction) were too large by almost a factor of two and was on the order of 30″. (Brown 1960, 160, 258-259)

Tables based on Hansen’s theory were not published until 1857. At the same time, Hansen wrote a series of articles rederiving his theory in a more straightforward way to confirm the method, making his final contribution in 1864. The tables based on Hansen’s theory were the most accurate yet produced. In general the tables were accurate to about a second of arc. They would remain the standard reference on the Moon’s motion for the next fifty years. (Linton 2004, 409-412; Brown 1960, 161; Gutzwiller 1998, 620)

Another significant accomplishment in lunar theory was the work of Charles Delaunay (1816-1872). Delaunay’s theory was published in *Les Mémoires de l’Academie des Sciences* in 1860 and 1867 in two large volumes totaling 1800 pages. He had first proposed the method he employed in 1846. The derivation of the results took Delaunay twenty years working alone, ten years for the actual derivation and ten years checking his calculation. A twentieth century computer analysis of his equations would find only three errors in Delaunay’s algebra, two of which were a consequence of the first. The expression for the Moon contained over 800 trigonometric terms taking several parameters to the eighth order. (Linton 2004, 411; Brown 1960, 133; Gutzwiller 1998, 618; Pavelle *et al.* 1981, 151)
As with Hansen’s theory, the variation of arbitrary constants was the basic method, but Delaunay’s theory was purely literal with no numerical values substituted until all the algebraic and analytical manipulations had been performed. Also, Delaunay replaced some of the standard Kepler parameters with others more suited to the calculation. The work up to 1867 accounted only for the effects due to the complex interactions of the Sun-Moon-Earth system, with all bodies treated as points, and the orbit of the Earth-Moon system around the Sun treated as an unperturbed elliptical orbit. This is the so-called main problem of lunar theory. Delaunay had intended to include the perturbations due to the planets, but he did not finish this work. He died in 1872 by drowning, leaving others to finish the work. It was shown that Delaunay’s theory had the same inherent accuracy as Hansen’s theory. It was not until 1911 that tables based on Delaunay’s work were published and briefly used for the production of astronomical and navigational tables. (Brown 1919, v; Brown 1960, 133, 159; Gutzwiller 1998, 616-617)

In 1874, Sir George Biddell Airy (1801-1892), Astronomer Royal of England, suggested a novel method for improving on existing lunar theory. He began with Newton’s equations for force (force equals mass times acceleration) and gravity for a three body system. The resulting equations for the Earth-Moon-Sun system are given as:

\[
\left\{ \frac{d}{dt} \left( \frac{r}{a} \cos l \right) \right\}^2 + \left[ \left( \frac{r}{a} \cos l \right)^2 \left( \frac{du}{dt} \right)^2 \right] - \frac{1}{2} \frac{d^2}{dt^2} \left( \frac{r}{a} \cos l \right)^2 \right) - \frac{\epsilon + \mu}{r} \left( \cos l \right)^2 + \Omega + A_n = 0 \tag{2.1}
\]

\[- \frac{d}{dt} \left[ \left( \frac{r}{a} \cos l \right)^2 \frac{du}{dt} \right] + \Omega + A_n = 0 \tag{2.2}
\]

\[- \frac{d^2}{dt^2} \left( \frac{r}{a} \sin l \right) - \frac{\epsilon + \mu}{r^2} \sin l + \Omega + A_n = 0 \tag{2.3}\]

Here \( r \) is the distance between the Earth and Moon, \( a \) is the average distance between the Earth and Moon, \( l \) is the Moon’s latitude, the angle between the Moon and the ecliptic, \( u \) is the longitude of the Moon, \( t \) is the time, \( \epsilon \) is the mass of the Earth, \( \mu \) is the mass of the Moon, \( \Omega \) is the acceleration due to Solar attraction and \( A_n \) are additional force terms. The best available equations of position for the Moon (i.e. the best current
theory of the Moon) would be substituted into equations 2.1-2.3 with a modification. A correction term (also called a residual) would be added to the coefficient of each term in the best current theory. This set of equations of position would be substituted into the differential equations of motion for the Sun-Moon-Earth system (Eq. 2.1-2.3). (Airy 1886, 8-12) In order to calculate the corrections the coefficients must be in numerical form and so in that sense Airy’s theory is a numerical theory, although it still gives a closed form algebraic solution.

If the equations of position were perfectly in accord with the equations of motion, Newtonian theory, then all the terms would cancel out leaving the residuals equal to zero. In practice the residuals would not be zero. The residuals would first act as a confirmation on the solution’s theoretical precision. The residuals could then be used to fit a new set of terms of greater precision, and hopefully greater physical accuracy. The method is in many ways straightforward, however it required a great deal of calculation and algebra. Airy proposed to use Delaunay’s theory for the equations to be corrected and Airy published the work and results in *Numerical Lunar Theory* in 1886. Unfortunately a major omission invalidated the entire work, which had suggested suspiciously large corrections to Delaunay’s theory. Airy had assumed the existence of a spurious solar term that differed between Earth and Moon, dependent on a term M proportional to $\frac{e+\mu}{a^2}$. By this point Airy, 85 years old, did not have the energy or resources to rework his study. This method remained sound in principle, but was left to languish for decades. (Airy 1888a, 254; Brown 1960, 245-246; Radau 1887, 274-286) The later implementations of this method will be taken up in this thesis in subsequent chapters.

In 1877, American George William Hill (1838-1914) provided a key development for both predictive and qualitative astronomy. He developed Euler’s idea of a lunar theory using rectangular coordinates. Hill chose a reference frame where the axis had the same motion as the mean motion of the Sun relative to the Earth. Hill also recognized that $m$, the ratio of the mean motions of the Sun and Moon, was the slowest parameter to
converge in literal methods. So in the rectangular coordinates he worked out equations for the portion of the Moon’s motion depending on $m$. The idea was to then take the numerical value of $m$ from empirical observation avoiding the slow convergence and related issues of small divisors in its series expansion. (Gutzwiller 1998, 619-621; Brown 1960, 195-198)

The orbit he derived for this is called the variational orbit and is an exact periodic solution of the three body problem. The name “variational orbit”, also “variation curve”, refers to “the variation” of the Moon discovered by Brahe. This should not to be confused with variational methods where equations are selected based on minimizing some property. The variational orbit consists of a circular orbit for the Moon plus the variation with no other terms included. For $m$ equal to the actual Sun-Earth-Moon system the variational orbit was a slight oval, and Hill and others explored the consequence of varying the period of the Moon’s mean motion. The variational curve could be used as the basis for an approximate orbit of the Moon by adding perturbations. Note that the variational orbit is not a good approximation to the Moon’s actual orbit on its own and lacks several corrections that were discovered in classical times. (Gutzwiller 1998, 621; Brown 1960, 125, 196)

Hill’s work was anticipated to an extent by the methods of Leonard Euler and his son Johan Albrecht Euler (1734-1800), who worked with him on his second lunar theory. Euler’s second lunar theory like Hill’s proposal used rotating rectangular coordinates. Hill also followed the Eulers in grouping the terms in the expansion of the problem by the parameters on which they depended. Finally Hill’s decision to use the variational orbit as the starting points recalls a statement by Johan Euler that a perfect solution for the variation would allow the solution of the entire lunar problem. (Gutzwiller 1998, 620; Wilson 2008, 453)

Hill never produced a complete lunar theory from these researches. However, in calculating the perigee of the Moon from the variational orbit he discovered that the
calculation depended on the determinant of a matrix with an infinite number of elements. He was able to give a series solution for the problem but could not prove convergence. Although this was a novel development, it turned out that John Adams had come to a similar set of relations, including an infinite determinant in calculating the node of the Moon, years earlier. Adams only published his results in response to the publication of Hill’s work in 1877. (Brown 1960, 196)

One other important aspect of Hill’s work was his consideration of certain invariant dynamical properties of the Sun-Earth-Moon system. Assuming the motion of the Sun and Earth to be simple circular orbits, one can derive the Jacobi integral:

\[ KE = 2F - C \]

where KE is the kinetic energy of the bodies, F is the sum of the forces acting on the three bodies, and C is an arbitrary constant. The integral is derived in a coordinate system where the x and y axes have their origin at the centre of gravity of the Sun and Earth. The x-axis passes through both the Sun and Earth and rotates with them. Using this integral and assuming the approximation held, Hill established that the Moon would not escape its orbit around the Earth. His derivation rested on the ability to define a boundary to the motion, where \( C = 2F \) the motion of the third body (the Moon) relatively to the other two is zero. Thus a surface can be drawn, in the rotating coordinate system, where \( C = 2F \) that defines the limits of motion of the Moon. The Jacobi integral can also be used to derive various general properties of three body systems. For example, the local maximum or minima for F are the Lagrange points. (Linton 2004, 413-416; Brown 1960, 25-26)

Hill’s original and important work would influence the course of development in celestial mechanics in the following decades. Henri Poincaré described it in the following way: “In this work... one may note the germ of the greater part of the progress that
the Science has made since.”¹¹ (Quoted in Brown 1960, 196) This impact would be seen both in accurate predictions of the Moon’s position and in understanding more general properties of orbits.

The first of these developments was the work of Poincaré himself. Henri Poincaré is perhaps the most prominent applied mathematician of the late 19ᵗʰ century. In the 1880s he wrote on the qualitative analysis of differential equations. His work in celestial mechanics began as a response to a prize competition set up to celebrate the sixtieth birthday of King Oscar II of Sweden and Norway (1829-1907) in 1889. Poincaré submitted an essay on the Three Body Problem. Inspired by Hill he looked at how orbits developed close to exact periodic solutions of the three body problem. Like Hill, Poincaré made use of the Jacobi integral, in this case to create a phase space representation of the solutions under study. Poincaré showed that the series solutions employed in celestial mechanics will not in general converge. This is, in part, because two trajectories with initial conditions very close to each other can develop vastly different motions over time. Any approximate solution covers some range of actual physical situations and so covers diverging physical conditions; therefore by converging with some actual solutions an approximation must diverge from others. Poincaré’s essay won the prize and was published in 1891. (Linton 2004, 419-421, 426-427)

Poincaré continued working on questions in celestial mechanics publishing two multi volume books on the subject. The first *New Methods in Celestial Mechanics*, published in three volumes between 1892 and 1899, examined in detail the issues raised by his prize winning essay. He developed various theorems and techniques for examining dynamical questions and examined the convergence of various kinds of series. The second work *Lectures on Celestial Mechanics*, also in three volumes published between 1905-1910, examined, among other things, the mathematical techniques used in celestial mechanics

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¹¹Dans cette œuvre... il est permis d’apercevoir le germe de la plupart de la progrès que la Science a faits depuis. The translation is my own.
In doing so Poincaré also pointed to limits on the mathematical convergence of the methods. Many of the series used in celestial mechanics do not converge in the mathematical sense of tending to some predetermined limit for the sum of all the infinite terms. In practical applications a series that does not converge for all terms may do well for the first several. On the other hand a series, mathematically established to converge asymptotically, may require too many terms to achieve the desired accuracy given limited time and resources. Poincaré’s work demonstrated that the existing “proofs” of the solar systems stability were founded on untenable assumptions about how well behaved such series would be. (Linton 2004, 427-432; Gutzwiller 1998, 619)

In the later part of the nineteenth century, other areas of celestial mechanics continued to develop. A significant event was Simon Newcomb’s (1835-1909) appointment to the Directorship of the Nautical Almanac Office. This office was responsible for producing navigational and astronomical tables for the United States. Newcomb used the institutional resources of the Office to organize a project to standardize the various planetary theories. (Linton 2004, 412)

Since the planets perturbed each others’ orbits, their physical properties, particularly the mass, were parameters in the equations for the motions of the other planets. Urbain Leverrier (1811-1877) had worked out the best planetary theories for most of the planets and, in order to achieve a better fit, he would optimize the values of the various planetary masses to give the best agreement with observations for each planet’s motion. Therefore, the same planet would be accorded a different mass in each theory. This was in part due to the real uncertainty about planetary mass at the time. In any case Newcomb’s project was to create a set of planetary theories where the masses used for each planet would be consistent throughout. (Linton 2004, 406-407)

Together with G. W. Hill, Newcomb developed a set of planetary theories marginally more accurate than Leverrier’s. At the same time, researchers associated with Newcomb
and the Office carried out other important research. All this helped raise the profile of American astronomy at the end of the 19th century. (Linton 2004, 406-407, 412)

One other minor but noteworthy development in 19th century celestial mechanics should be noted. In 1889, noted American physicist, Josiah Gibbs (1839-1903) wrote a paper on orbit determination for asteroids from three observations using vectorial methods. He wrote the paper as an example of the utility of vector methods, which were not widely used at the time. The method was soon implemented by others. Gibbs received 199 requests for reprints, the second largest number of requests for any of his papers, suggesting its popularity. (Crowe 1985, 160) The current ubiquity of vector methods can make it hard to imagine how scientists worked on such problems before their advent. As with Leibniz’s formulation of calculus, vectorial methods suggest that the notation and formalism of mathematics, rather than an external device, can be a computational aid.

Hill’s lunar theory would be turned into a viable means of predicting lunar motions by Ernest William Brown. Brown had become interested in Hill’s lunar theory while a Masters student at Cambridge working with George Howard Darwin (1845-1912), son of famed naturalist Charles Darwin (1809-1882). George Darwin, Plumian Professor of Astronomy and Experimental Philosophy, was interested in the dynamical theories of Poincaré and would make his own contribution to the study and classification of periodic orbits in 1897. It was Darwin who advised Brown to study Hill’s work. (Schlesinger and Brouwer 1941, 243-244; Linton 2004, 424-425)

Brown began his researches by reviewing all of lunar theory. The result of this was his 1896 An Introductory Treatise on the Lunar Theory, a technical summary of the main aspects of various lunar theories especially those of de Pontécoult, Hansen, Delaunay and Hill. A short section near the end considers the pros and cons of the various techniques for deriving an improved lunar theory and reveals a preference for Hill’s techniques. One of the stated advantages of Hill’s method is that it can be carried out to a greater extent by a (human) computer capable of only following definite rules compared to other
In taking on the lunar theory, Brown described his purpose as follows: “to find in the most accurate way and by the shortest path the complete effect of the law of gravitation applied to the Moon.” (Brown 1914, 185) In all, Brown’s theory contained over 1400 terms. The development of these equations and their publication took Brown over 14 years from 1894 to 1908. He worked with the assistance of one computer, Mr. Ira I. Sterner. (Schlesinger and Brouwer 1941, 245-247; Comrie 1932, 694; Brouwer 1939, 302) Brown’s work would not be surpassed for decades.

Brown’s work on lunar motion did not end with the publication of his theory. In order to render calculations from his theory manageable for practical purposes, Brown produced *Tables of the Motion of the Moon* in 1919 after more than a decade of further work. This time he was aided by Dr. H. B. Hedrick (1865-1936), an experienced computer at the American NAO. Finding the numerical values for the constants in the theory was aided by the work of Philip Herbert Cowell (1870-1949), a British astronomer who worked at HMNAO. Cowell reduced 20,000 observations from the Greenwich observatory between 1750 and 1900 for comparison with theory. (Schlesinger and Brouwer 1941, 247-249)

Brown's *Tables* were an aid to calculation and not a list of positions or ephemerides. They included all the information necessary to find the sum of the series that made up Brown’s theory of the Moon and so give the position of the Moon. The calculations implicit in the *Tables* converted Brown’s results to spherical polar coordinates, set values for the physical constants used in the equations and reduced the 1400 plus terms of the equations into functions whose values could be found in the 180 tables spanning 660 pages and three volumes. Brown also made every effort to ensure that the tables were laid out in a way that made them easy and quick to use. In principle, calculation of the Moon’s position became a matter of looking up the values in the tables and adding them together. In practice, from 1923, Brown’s tables were used by various national institutions like His Majesty’s Nautical Almanac Office (HMNAO) to produce ephemerides, books with daily
values of the position of the Moon, among other things. (Comrie 1932, 694; Schlesinger and Brouwer 1941, 247-249)

Brown’s theory increased the accuracy of the prediction of the Moon’s motion by at least an order of magnitude (0\".1), allowing it to match observation and confirm again the accuracy of Newtonian theory. Brown’s theory captured the contributions to the Moon’s motion from the Earth and Sun’s gravity to the order of about 0\".01; the planetary perturbations were not necessarily as accurately represented. One of the lunar theory’s most striking impacts was to foster acceptance that the Earth’s rotation has an irregular variation in its period that explained several anomalies in the motion of the Moon and other bodies. This eventually led to the establishment of “ephemeris time”, independent of the Earth’s rotation and based on the orbit around the sun. (Schlesinger and Brouwer 1941, 246-249)

2.6 Machine Computation

A digital calculator is one that manipulates discrete numbers, represented by things like geared wheels, with appropriate numbers painted on them, counting wheels. The first digital mechanical calculators documented by history, developed in the 17\textsuperscript{th} century, were capable of addition, subtraction and multiplication of given numbers. Such machines saw little practical application until the 19\textsuperscript{th} century because of their expense and tendency to break. This was despite the desire of their inventors, mathematicians, to eliminate the drudgery of long calculations. For example, Leibniz invented such a calculating machine and at one time commented that long calculations deterred astronomers from their work and that: “For it is unworthy of excellent men to lose hours like slaves in the labor of calculation which could be safely relegated to anybody else if machines were used.” (Quoted in Goldstine 1993, 8) The slide rule was invented soon after the introduction of logarithms, but these remained rare instruments until the 19\textsuperscript{th} century
and lacked the precision required for most astronomical work. Tables of logarithms and other functions remained the main aid to calculation for the human computer for two centuries. (Goldstine 1993, 4, 7-8; Grier 2005, 92)

Even in the nineteenth century mechanical calculators saw little use in science, despite growing use in business. However, by the turn of the 20\textsuperscript{th} century several people advocated the use of desktop adding and calculating machines in science, by writing articles in scientific publication on their use and potential benefits. More concrete evidence of the move to mechanical calculators and away from logarithm tables can be seen in the tables of trigonometric functions. With the widespread adoption of logarithms as the means of multiplication, tables of trigonometric functions gave the values of the function as a logarithm. German publishers produced tables of trigonometric functions with the actual values as early as 1907 suggesting a move away from logarithms by at least some workers. Despite this, many scientific computing institutions, such as the compilers of the national ephemerides, continued to use logarithms into the 1930s. (Croarken 1990, 12-21)

A famous misadventure in mechanical calculation occurred during the first half of the 19th century. Englishman Charles Babbage (1791-1871) conceived an extraordinarily ambitious project of machine computation. Babbage proposed to build a computation machine which would eliminate human error from the process of astronomical table making and thus promising a new level of reliability for the results. Charles Babbage’s scientific work was chiefly in mathematics, but he was constantly lobbying for the reform of science in his native England. His first efforts towards this were the translation of various French and other continental works on analysis. He and other students, who constituted the members of the Analytical Society at Cambridge, were convinced a devotion to Newton’s notation for calculus had held back developments in England. Babbage also advocated for greater government support of science and scientists, feeling again that the continental Europeans had a superior system. (Swade 2000, 18, 62-65)
Babbage first publicly announced his proposal for a machine in 1822, when he unveiled a small prototype. He proposed a larger machine which he called the difference engine; a machine that would calculate the values of a function at successive fixed intervals of the argument using the method of finite differences. The difference engine would also print the results directly to prevent transcription errors. (Swade 2000, 25-32)

Since the method of finite differences comes up in various calculations important to this thesis, it is worth reviewing it here. The first differences of a function are the differences between the value of the function at fixed intervals of the argument, the second differences are the differences between successive first differences and so on. For any polynomial function of finite order, there will be some $n^{th}$ difference where all $n^{th}$ differences have the same value. All higher differences are by definition zero. This is because taking the difference approximates taking a derivative, repeating the operation of taking the derivative on a polynomial will reduce its order until it becomes zero (a constant).

Therefore, given a starting value and associated set of differences up to this constant, subsequent values of the function can be obtained by adding the differences to obtain the next difference and so on down the line until the subsequent value of the function is found. This is the simplest method of extrapolation with differences and the one Babbage proposed using, but there are other more complex formulas. Even if the differences are not taken to the constant difference, simply treat a set of sufficiently high order of the differences as if they are all zero and this method will approximate the values of the function. Taking the $n^{th}$ difference as constant (all higher orders as zero) is equivalent to treating the function as a polynomial of order $n$, and any function can in principle be approximated by a finite polynomial (a truncated geometric series). On this basis Babbage argued the machine would be widely applicable. (Swade 2000, 29; Lardner 1961, 184-188)

The method of differences had been used in the production of mathematical and
scientific tables by others, long before Babbage. The example Babbage himself used was
the cadastral tables of revolutionary France organized by the Baron Gaspard-Marie Riche
de Prony (1755-1839). The Bureau de Cadastre was concerned with surveying and register
land and the new units of measure instituted by the revolutionary government required
new tables. The intended computational work consisted of tables of logarithms and
trigonometric functions and totalled 18 volumes. Baron de Prony had taken advantage
of the large number of unemployed servants of the deposed aristocrats to do the work.
Inspired by Adam Smith’s description of factory work, de Prony sought to break down
the work of calculation so that the basic steps could be done by workers ignorant of more
than basic arithmetic. Such simple procedures would make the transition to a machine
method easy. Babbage was not the first to suggest the method of differences could be done
by machine. A German, Johann Helfrich Müller (1746-1830) had suggested mechanizing
the calculation and printing of mathematical tables in 1784. Although, Müller’s work
was probably unknown to Babbage. (Swade 2000, 30-32; Grier 2005, 36-37)

In addition to producing tables, the method of differences could also be used to
find errors in existing tables. By taking the differences of successive values in the table
and then differences of those differences and so on. Any error should show up as an
anomalous difference. By the 20th century this had become a common and popular
means of verifying the accuracy of tables and finding errors. (Buxton 1933, 60)

Two things make Babbage’s difference engine of particular note. Apparently his ini-
tial inspiration for mechanizing table making was the tedious experience of correcting
the work of human computers for a set of astronomical tables with his friend, and noted
astronomer, John Herschel (1792-1871) (son of Uranus’s discoverer). According to the
anecdote the profusion of errors led him to exclaim. “I wish to God these calculations
had been executed by steam.” (Swade 2000, 9) Second, he emphasized the construc-
tion of astronomical tables as a primary application of his difference engine. (Babbage
1961a, 311-312) Babbage’s quest to build calculating machines and the interest of his
contemporaries suggest that demand existed for better ways to carry out the voluminous calculations required for accurate prediction in celestial mechanics.

Babbage spent years and large sums of money, his own and the British government’s, trying to make the difference engine a reality. After expending more than £17 000 of government money, the project work stopped in 1833 due to disagreements with his engineer and workmen. (Swade 2000, 66-67) In the process of attempting to build the difference engine Babbage conceived of an even greater machine. He would call this machine the analytical engine. A machine that was capable of carrying out any series of arithmetic operations and even varying the operations in response to intermediate results, all controlled by an indefinite series of punched cards. This machine would never be built either, but almost a century later it would become his greatest claim to fame because of its resemblance to the modern computer. (Swade 2000, 109-110, 169-171)

On a more humble scale, reports of Babbage’s difference engines inspired the Swedish father-son team of Georg (1785-1873) and Edvard (1821-1888) Scheutz’s to build their own difference engine between 1833 and 1853. Their machine worked to only four orders of difference, where Babbage’s would have had at least five orders and he had a later design that would allow for seven. Two machines would be built by Scheutz. The first was purchased by the Dudley Observatory in Albany, New York in 1857. A second was purchased in 1859 by the Registrar General’s Office of England to compute the English Life Tables of 1864. Both machines were plagued by mechanical faults. The British machine was only used for the 1864 life tables. The American machine was used briefly by workers at the American NAO to interpolate values for part of the orbit of Mars in 1858. The Almanac works found it too unreliable and difficult to set-up to justify further use. (Swade 2000, 45, 193-209; Grier 2005, 68-70) It would be decades before mechanical calculators would become a common tool in scientific calculations or table making.

When Babbage died in 1872 he was still well remembered by his fellows, but only his son, retired Major-General Henry Provost Babbage (1824-1918), would directly take up
his work. Those who specialized in mechanical calculation would continue to remember
him and find inspiration in his work. (Obituary 1872; Babbage 1910; Ludgate 1914) How-
ever, the use of calculating machines in science came from another avenue, commercial
machines.

Thomas de Colmar’s (1785-1870) calculating machine, sold under the brand name
Arithmometer, was the first commercially successful machine. First built in 1820, the
Arithmometer was sold in various versions to the end of the century, and beyond, but
was not produced in large numbers until the second half of the 19th century. By the 1880s
several calculating companies had formed and new features were being added to machines
on a regular basis, such as direct multiplication and printing of results. One important
advance was operating the machine by keyboard rather than switches or dials. One of the
first popular and successful machines to employ a keyboard was Eugene Felt’s (18621930)
Comptometer, invented in 1884. By the 1890s there were more than a dozen companies
selling adding or calculating machines with some of them selling hundreds of machines.
The primary customer of these machines were businesses, but some astronomers were
apparently using these machines by the 1880s. (Cortada 2000, 25-43; Williams 1990,
50-51; Grier 2005, 93)

One other kind of calculating machine of note has its origins in the 19th century. In
the 1880s, Herman Hollerith developed machines for calculating the statistics of the US
census. He went on to develop the machines for wider business uses in the 1890s. These
machines used punched cards and would automatically add (to a mechanical counter)
in response to the presence of a punch in the appropriate portion of the card via the
completion of an electric circuit. Hollerith also developed a machine to sort the cards on
the basis of which hole had been punched. This technique would be developed and im-
proved to allow automatic feeding of cards through the machine, the adding of quantities
specified by punches on the cards, later subtraction and eventually multiplication. These
machines were known variously as Hollerith machines, punched card machines and IBM
The use of punched card machines in scientific computing was slow to catch on. Although there is some indication Hollerith machines were used briefly in the 1890s to compile statistics for the weather bureau of the United States, in general such machines did not see much use in science until the 1920s. (Nebeker 1995, 92-93) These applications will be discussed in the next chapter because of Eckert’s involvement.

As illustration of how early mechanical calculators were used and because of its relevance and possible influence on later events, I will discuss in detail E. W. Brown’s use of calculating machines in his lunar work. Brown was acutely aware of the practical issues associated with astronomical tables. He was aided in designing them by Hedrick, an experienced maker and user of astronomical tables. Brown was also clearly looking for new ways to calculate. During the gestation of the tables, Brown publicly raised the question of whether tables should be designed so that they might be easily used with mechanical aids like calculators and typewriters. He also mused about special machines to help shorten the work of the ephemeris computer. (Brown 1909, 149)

In fact, Brown employed a novel device and machine method for producing some of the numbers in his tables. The device is mechanically simple (see figure 2.1). It consists of a rectangular wooden frame; a series of guides (made of knitting needles) strung across the frame; a brass rectangular carrier (thin rectangular piece of metal) laid perpendicular to the guides and closed loops of made of fabric, cambric, hanging on the carrier. Pieces of cardboard with numbers written on them are pasted on the loops. The pieces of cardboard (cards) are about equal in length to the carrier’s width so that they rest on it comfortably. The cards are spaced such that turning over the carrier puts a new set of cards on top of the carrier. The whole thing is hung over the side of the operators desk as shown in figure 2.1. (Brown 1912, 455)

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12 The descendant of Hollerith’s Tabulating Machine company became International Business Machines (IBM) in 1924.
Brown’s Device for Calculating with Harmonic Functions. (Brown 1912)

Figure 2.1

The purpose of the device was to supply the operator of a mechanical adding machine with the numbers he needed to add for the evaluation of a harmonic series at fixed intervals. The harmonic series was of the form:

$$f(x) = A\sin(ax) + B\cos(bx)\ldots$$

Such a summation depends on finding the numeric value of the component functions for a given value of the argument (e.g. $x=5$) and adding them. The successive values of a function were printed on successive pieces of cardboard on a loop. Each strip contained the values for a different function. Initial set-up consisted in hanging the loops so that the numbers displayed on the pieces of cardboard sitting on the carrier were the values required to calculate the first sum of the harmonic series. By simply turning the carrier the next set of numbers on the loop is displayed and these are the numbers to be summed to obtain the next sum. The numbers are in loops because they record the values of harmonic functions that repeat after some point.
Suppose for example that one wanted to add the two terms of the series:

\[ f(x) = 2\sin(0.5x) + 3\cos(x) \]

at intervals of 5° for \( x = 0° \) to 720° using the machine. You would prepare two loops, one would have 144 pieces of cardboard and the numbers: -0.0872 \([=2\sin(357.5°)]\), 0 \([=2\sin(0)]\), 0.0872 \([=2\sin(2.5°)]\)… would be on successive pieces of cardboard on the loop. The other loop would have 72 numbers on it: 2.9886 \([=3\cos(355°)]\), 3 \([=3\cos(0)]\), 2.9886 \([=3\cos(5°)]\)… in succession. The machine operator (computer) would start by hanging the loops so that the value for \( x = 0° \) was showing (ie 0 for the first and 3 for the second). The computer would add the two visible numbers and record the sum. The computer would then turn over the carrier revealing the next two numbers to be added (0.0872 and 2.9886) and add them. He could continue this 143 more times to obtain all the sums.

In spite of some complications, Brown felt that the system allowed the calculations to be carried out by an individual of no special skill or ability. For the adding machine Brown favoured the Comptometer brands because its key driven mechanism made it particularly easy and fast for the operator to use. (Brown 1912, 454, 457)

Brown reported using the technique in the compilation of tables for his *Tables of the Motion of the Moon*. In addition to the device described and the adding machine, Brown also employed a Burroughs adding and printing machine. The Burroughs machine was used to print the results. The machine also provided a check that the right numbers had been used by checking the sum of the results. (Brown 1912, 460)

The addition of terms in a trigonometric series at equal intervals was also the operation required for the production of the values used by an ephemeris. Brown suggested another application was the analysis of a large mass of data collected at equal time intervals, as in analysis of the heights of the tide. Apparently independently of Brown, two X-ray crystallographers H. Lipson (1910-1991) and C. A. Beevers (1908-2001) developed a similar method for harmonic analysis. Lipson and Beevers, like Brown, had the values
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of trigonometric functions at fixed intervals printed onto cardboard. Instead of loops the Beevers-Lipson strips were long rectangles; the numbers of a function lay flat next to each other. By properly setting up a series of strips above and below each other, the sum of a series could be found by adding up a column and each successive column was the value of the sum at the successive interval. The Beevers-Lipson strips were used with problems requiring many fewer significant digits than Brown’s problems and so did not have to be as long as Brown’s loops. There is no evidence that Beevers or Lipson had any knowledge of Brown’s earlier work. (Brown 1912, 456, 460; Lipson and Beevers 1936, 775-776, 779)

Brown’s lunar tables were used by the British HMNAO in order to produce vital navigational and astronomical ephemerides. However, well into the 1920s the principal method employed by the HMNAO for aiding in extensive calculations was the logarithm table. The HMNAO did have a modified Burroughs adding and printing machine from about 1914, but this was only used for a small subset of their work. It was only in 1925, with the arrival at the HMNAO of Leslie John Comrie, that the use of calculating machines at the HMNAO became widespread. (Croarken 1990, 22-24) His work will be discussed in the next chapter.

2.7 Other Early 20th Century Developments

Undoubtedly the most famous incident in early twentieth century astronomy is the explanation of the 43” per century advance in Mercury’s perihelion by Einstein’s General Theory of Relativity. The discrepancy of Mercury’s orbit was only clearly identified by its recalcitrance to correction by Leverrier and Newcomb’s efforts. The incident showcased the ability of celestial mechanics to act as a fundamental and sensitive test of the properties of gravity. Previously this had acted to strengthen Newtonian gravity, now it refuted it. However, for most heavenly bodies the deviation from Newtonian behaviour
is incredibly slight. As a result it was still possible to formulate and solve problems in celestial mechanics using Newtonian equations for much of the twentieth century. If necessary, the effects of general relativity could be added as a final perturbation. (Linton 2004, 458-472)

One other development in early twentieth century astronomy needs to be discussed. P. H. Cowell, working for HMNAO along with Andrew Crommelin (1865-1939), found the orbits of Jupiter’s eighth moon in 1908 and the returning path of Halley’s comet for 1910. Both orbits are highly perturbed by gravitational effects from more than one large body, so in both cases Cowell and Crommelin employed numerical integration. In recognition of Cowell’s efforts the method became known as Cowell’s method. (Grier 2005, 119-125)

Numerical integration involves solving an integral using a set of numerical approximation schemes. In a real sense the method of differences itself is a numerical integration technique, the differences approximate derivatives and their summation, to derive lower order terms, approximates finding the definite integral. Essentially using differences in their approximate form finds the integral as approximated by a polynomial of the same order as the highest difference used. Higher order differences may be needed, since a lower order polynomial may not be a good approximation of the integral. Finding higher order differences requires more terms in the function to be known and, since this is precisely what one wants to find this method has limited appeal.

In the case of planetary orbits, for any given position an acceleration can be calculated by Newton’s inverse square law, since acceleration is the second derivative of position it follows that the position can be found as the double integral of the acceleration. The schemes employed in Cowell’s method take advantage of this to find the differences of the acceleration instead of simply the difference of the position. Also, Cowell’s method takes advantage of more complex relations between differences and their functions.

Cowell’s method, as originally implemented, carried out numerical integration in three
phases that are iterated as often as needed. The process starts with all necessary differences and gravitational attraction calculated for $x$ at time $n$. First, a future acceleration term, for time $n+1$, is estimated by the simple method of differences. Second, a new position, for time $n+1$, is calculated, more accurately, using the basic formula of numerical integration. The formula for numerical integration is a series of arithmetic steps that approximates the integral $x(n+1) = x(n) + \int_{n}^{n+1} a(t)dt^2$, where $x$ is position, $a$ is acceleration, $t$ is time and $n$ is the initial time. From this new position term a new acceleration term is calculated. In the third phase the differences are recalculated in light of the newly calculated position and acceleration. The cycle can now be repeated again to find the position at time $n+2$.

In fact the techniques in the cases of Jupiter’s satellite and Halley’s comet were slightly different. This formula is derived by treating the position as the function of a truncated polynomial series:

$$x(n + t) = x(n) + a_1 t + a_2 t^2 + a_3 t^3 + a_4 t^4 + a_5 t^5 + a_6 t^6 ... \quad (2.4)$$

$$x(n + 1) = 2x(n) - x(n - 1) + 2a_2 + 2a_4 + 2a_6 ... \quad (2.5)$$

where $x$ is the position of the object, $n$ is the initial time, $a_n$ are unknown parameters and $t$ is the time passed.\(^\text{13}\) Then take the second derivative of 2.4 with respect to time and find the acceleration terms:

$$\frac{1}{12} [X(n + 1) - 2X(n) + X(n - 1)] = 2a_4 + 5a_6 ... \quad (2.6)$$

\(^\text{13}\)The intermediate steps are:

\begin{align*}
  x(n + 1) &= x(n) + a_1 + a_2 + a_3 + a_4 + a_5 + a_6 ... \\
  x(n - 1) &= x(n) - a_1 + a_2 - a_3 + a_4 - a_5 + a_6 ... \\
  x(n + 1) + x(n - 1) &= 2x(n) + 2a_2 + 2a_4 + 2a_6 ... 
\end{align*}
where $X$ is the double derivative of $x$ and $X(n) = 2a_{2}$.\textsuperscript{14} Equation 2.6 thus gives a relationship between the second difference in acceleration divided by twelve and the unknown parameters of the solution. Let the second difference be designated $X''$. Thus substituting for the various terms the equation to integrate the position one interval forward is:

$$x(n + 1) = x(n) + [x(n) - x(n - 1)] + X(n) + \frac{1}{12}X''(n)$$  \hspace{1cm} (2.7)

with the error of $3a_{6}$ assumed to be small. Equation 2.7 is the formula for numerical integration in this version of Cowell’s method. This was the relationship used by Cowell and Crommelin to generate an orbit for the eighth satellite of Jupiter. The acceleration terms were calculated from the positions by use of Newtonian gravity, the inverse square law. Note that $X''(n)$ can not actually be calculated since it requires the next acceleration term, $X(n + 1)$. First the position is estimated from past values and this estimate is used to generate a tentative value for $x(n + 1)$ from which a better $X(n + 1)$ estimate can be generated to calculate the final values. (Cowell and Crommelin 1908, 577)

For the case of Halley’s comet, Cowell and Crommelin devised a more complex integration scheme than equation 2.7 that included two more higher order terms. The first formula they generated and used for the calculations was as follows:

$$x''(n + 1) = X(n) + (1/12)X''(n) - (1/240)X^{IV}(n) + (1/1951)X^{VI}(n) + ...$$  \hspace{1cm} (2.8)

where $X^{IV}$ is the fourth difference of the acceleration and so on, a new value of $x$ is calculated from the second difference $x''(n + 1)$ and previous differences.\textsuperscript{15} This version is

\textsuperscript{14}The intermediate steps are:

\begin{align*}
X(n + t) &= 2a_{2} + 6a_{3}t + 12a_{4}t^{2} + 20a_{5}t^{3} + 30a_{6}t^{4}...
X(n + 1) &= X(n) + 6a_{3} + 12a_{4} + 20a_{5} + 30a_{6}...
X(n - 1) &= X(n) - 6a_{3} + 12a_{4} - 20a_{5} + 30a_{6}...
X(n + 1) + X(n - 1) &= 2X(n) + 24a_{4} + 60a_{6}...
\end{align*}

\textsuperscript{15}Note that 1/1951 is a close approximation of $\frac{31}{60480}$. This formula can also be derived from the Bessel Interpolation function. (Danby 1992, 291-293)
Chapter 2. The Methods of Celestial Mechanics

sometimes called the “fundamental formula.” After completing their work they realized that a superior formula would have a similar form, but give the position directly. As with the much older method of Clairaut, the drudgery of Cowell’s method is apparent from the attempts to minimize the number of steps in the integration, by increasing their size in places where the motion is more regular. (Cowell and Crommelin 1908, 576-577; Cowell and Crommelin 1910, 8-10, 84)

Cowell’s prediction of the perihelion for Halley’s comet was April 17, accurate to within 3 days based on the sightings of the comet in 1835 and earlier epochs. However, the prediction was not published until 1910 and early observation of the comet in 1909 had already allowed the perihelion to be predicted more accurately by less esoteric methods. (Morando 1995b, 234) Cowell’s method would be used by Wallace Eckert in his work both with punched card machines and modern computers later in the 20th century.

Cowell’s method generated considerable interest among astronomers. For example, in 1917, Arthur Eddington (1882-1944) suggested an alternative derivation of the method. Russian astronomer Boris Numerov (1891-1941) was a vocal advocate of the method. In papers in 1923 and 1924, he suggested a technique involving non-standard coordinates in an attempt to reduce computation. In 1924, John Jackson (1887-1958), another British astronomer, gave notes on the method as used by Crommelin and Cowell. In particular Jackson noted that people seemed unaware of the latter refinements and continued using the “fundamental formula” that Cowell went on to criticize at the end of his work on Halley’s comet. Jackson also pointed out the use by Gauss of similar formulation in a different context. The method became known as Cowell’s method in astronomy, but it is also called the Gauss-Jackson method, presumably thanks to Jackson’s commentary. In 1926, Robert T. A. Innes (1861-1933) gave a thorough review of the method, its

\[ x(n+1) = x(n) + \frac{1}{12} X(n) - \frac{1}{240} X''(n) + \frac{1}{1951} X^{IV}(n) \]  \hspace{1cm} (2.9)

where \(X\) is the double summation of \(X\). Summation being the inverse process to taking the difference.
history and suggested adopting it for use in the national ephemerides. However Innes also complained of the lack of wide knowledge or use of the method. (Eddington 1917, 375; Numerov 1924, 592-593; Jackson 1924, 602-603; Danby 1992, 306; Innes 1926, 269, 289-290) It is difficult to say if this increased interest in numerical integration is due to the increase in machine computation, but that is one possibility.

Several points should be emphasized as we conclude this chapter. Three methods described here will recur again and again. The first is the Hill-Brown theory of the Moon. This theory began with Hill’s insight that using the variational orbit as the first approximation of the Moon’s orbit and incorporating the ratio of the mean motions of the Sun and Moon into the equations, at the beginning of the derivation, would allow an accurate theory to be derived. Brown made lunar theory his life’s work and turned Hill’s initial insight and work into a fully formed theory. In doing so, Brown created a new standard for accuracy. Brown also innovated in his attempts to make his theory easier to calculate.

The second important method is Airy’s method for lunar theory. The goal of Airy’s method was to avoid the long drawn out algebraic analysis of theories like Hill-Brown. However, Airy’s theory still required large amounts of relatively simple computation be done in order to find a solution. This, combined with Airy’s limited resources, led to the failure of this method to produce meaningful results. However, it held out the possibility of systematic and successive improvement to lunar theory.

The third method is Cowell’s method, which has just been described. As with Airy’s method, Cowell’s method replaced complex algebraic manipulation with straightforward computation. In fact Cowell’s method was even simpler than Airy’s. However, it also makes large demands on the computational ability of the human computer.

Finally one machine technology should be emphasized because of its recurrent role in this thesis. Hollerith’s punched card machines were the first to make it possible to carry out a series of computations without constant physical manipulation of a machine.
Simply by feeding cards through machines a computation could be performed. They also changed discrete mathematical information into a form manipulable by machine.

The history of celestial mechanics and its methods is long and varied. An often overlooked component of this history is the history of calculation and computation. The resources in time and effort to make calculations were a significant part of the work required to put forward new theories or maintain old ones. Techniques aiding computation, from new notation to the table of logarithms, often became integral to the practice of the science soon after their introduction. Also manifest was a desire for better ways to calculate and an ingenuity to use all the tools available. However, motive alone usually failed to bring about a technological solution. As we shall see, the adoption of the computer into celestial mechanics had a strong continuity with these earlier developments.
Chapter 3

Eckert Before the Computer

3.1 Eckert’s Early Life and Work

Wallace John Eckert was born in Pittsburgh, Pennsylvania on 19 June 1902. He was raised on his parents’ dairy farm in Albion, Pennsylvania. In later life his manner of speech was marked by his rural upbringing. The farm apparently held an appeal for Eckert throughout his life. In the 1950s and 60s, Eckert owned a farm in New Jersey where he would entertain his academic colleagues. (Tropp 1978, 128-129; Grosch MS 2007; Hergert 1975, 215)

Eckert received a Bachelor’s degree from Oberlin College, Ohio, in 1925. He then decided to pursue a Master’s degree in astronomy at Amherst College, Massachusetts, graduating in 1926. Having decided to pursue astronomy Eckert began a doctoral degree at Yale University in 1926. In the same year he became a junior instructor at Columbia University. Columbia did not offer graduate studies in astronomy. Eckert took courses at Columbia rather than Yale, however his graduate studies still required him to commute back and forth between New York and New Haven via train with some frequency. (Eckert MS 1967a, Part I, p. 4; Eckert MS 1940a; Tropp 1978, 128-129)

At Yale Eckert’s supervisor was E. W. Brown. Eckert wrote a dissertation entitled
“The General Orbit of Hector” completed in 1931 and published in 1933. Hector (Hektor) is a Trojan asteroid, named after one of Troy’s mythic defenders. The Trojan asteroids all have orbits near the Lagrange point (the point of a stable periodic orbit) between Jupiter and the Sun. After completing his lunar theory, Brown had taken up the study of the motion of the bodies near the Lagrangian points in general and later come up with a general method of obtaining the orbits for all bodies in the Trojan group. Eckert implemented Brown’s method for Hector. (Schlesinger and Brouwer 1941, 252-253; Eckert 1933, 161)

Eckert later reported that it became obvious early on that he would work in the exact astronomy of position. Apparently he was drawn to the high precision of this work either out of interest or aptitude. As a result Brown was a natural supervisor for this given the methodical and precise nature of his own work. Eckert told an anecdote about Brown that illustrated Brown’s dedicated nature. Brown would awaken as early as 3AM for “Celestial mechanics and coffee” even during his vacation periods. After breakfast around 7:30AM Brown usually spent the rest of his day on other work. Eckert recounted this story both in his obituary of Brown in 1939 and also to his student Harry F. Smith in the 1960s. This suggests Eckert connected with Brown’s meticulous efforts in celestial mechanics. On a more personal level Brown and Eckert shared a love of farms and farming. Brown owned and maintained a farm in Salem, Connecticut from 1914 onwards. (Eckert MS 1967a, Part I, p. 3; Eckert 1939, 65-66; Smith MS 2007; Schlesinger and Brouwer 1941, 259)

Eckert obtained the orbit of Hector in the form of trigonometric series for six parameters. He found that the method did not achieve much of a saving in effort and that he was unable to achieve as much accuracy as might be expected given the observations made of the asteroid. It is unclear what calculating aids he used in this work, but he would later note that he had a Monroe desktop calculator upon arrival at Columbia and that this was used in his thesis work. Also he stated that his training in astronomy included
the use of logarithms. (Eckert MS 1967a, Part I, p. 1; Eckert 1933)

Eckert published three short articles while working on his thesis. The first paper “Convenient Forms of Magnetometers”, co-written with S. R. Williams of Amherst College and published in 1928, was presumably an outgrowth of work he performed during his Master’s studies. The second paper, also from 1928, is a description of the astronomy department at Columbia for the journal *Popular Astronomy*, and the last, in 1931, is a general interest article on asteroids for *Natural History*, the journal of the American Museum of Natural History in New York. (Williams and Eckert 1928, 207; Eckert 1928; Eckert 1931) None of these is particularly technically challenging, but suggest perhaps a willingness to reach out beyond normal academic lines.

The article on the asteroids showed Eckert espousing the view that the motion of the heavenly bodies continued to provide an important test to scientists’ theories. Noting that there are over a 1000 known asteroids between Jupiter and Mars, he continued: “With such an assortment at hand it is possible to find one to test almost any theory.” (Eckert 1931, 26) This traditional view of observation as a test of theory would be echoed occasionally in his work.

Eckert also lectured at the American Museum of Natural History on various occasions, for example, once in 1930 on the “Island Universe” and once in 1937 on “Astronomical Calculations.” This was part of the continuing lecture series for the Amateur Astronomers Association of New York. In fact, Eckert is referred to as one of the “old favourites” of the series, by a later history. Many other astronomers spoke at these lectures, including, in 1930, E. W. Brown on “Time and Tide.” (Rizzo 1967; *New York Times* 1930, 38; *New York Times* 1937, 23)

In 1932, Walce Eckert married Dorothy W. Applegate (c.1904-?). Dorothy had worked as a computer and researcher in astronomy and in the 1960s would collaborate with Wallace in his work on Lunar theory. The Eckerts had two children. (Moore 1932, 191; Freeman 1971)
3.2 Comrie and Early Developments in Punched Card Methods

While Eckert calculated the orbit of Hector, rode the train from New York to New Haven and began his career in academic astronomy, developments were occurring elsewhere that would shape the trajectory of Eckert’s career. In 1928 L. J. Comrie began his most celebrated project at HMNAO, the full calculation of the positions of the Moon via punched card methods. This would be remembered as perhaps the first large scale scientific calculation undertaken using such methods.

Comrie had already written extensively on the use of calculating machines, mostly manual desktop calculators. In 1928 he published an article on interpolation. In this article, he noted the potential for punched card machines for using what he called the Bessel interpolation formula. This method has the form:

$$f(n) = f(0) + n\Delta'(1/2) + n\frac{(n-1)}{2} \cdot 2! (\Delta''(0) + \Delta''(1)) + n\frac{(n-1)(n-1/2)}{3!} \Delta''(1/2) + ...$$

where $\Delta'$ is the first difference, $\Delta''$ the second difference and so on, the arguments of the differences work as follows $\Delta'(1/2) = f(1) - f(0)$ and so for higher differences $\Delta''(0) = \Delta'(1/2) - \Delta'(-1/2)$. Comrie argued that this method allowed both efficient calculation and error checking by the use of the method of differences. Also, he felt that it was more suited to being done on mechanical desktop calculators and punched card machines than the Lagrange interpolation that was also popular. The Lagrange interpolation used a weighted average of values of the function at fixed intervals to calculate an intermediate value. (Comrie 1928b, 510-512; 520-522)

Comrie’s history making project was to find the positions of the Moon for the Almanac using punched cards. Brown’s Tables had made it possible to calculate a position just by looking up numbers in its tables and adding them together. Therefore Comrie had
to transcribe the numbers of Brown’s tables onto punched cards and then organize the
cards so that the machines would find the sums that gave the position of the Moon. Once
the cards were put in the right order, it would, in principle, just be a matter of feeding
them through the appropriate machine, a practically automatic process. The punched
card machine could even print the numbers on paper.

Comrie used the punched card machines of the British Tabulating Company, which
had the rights to the Hollerith patents in the United Kingdom. The first part of Comrie’s
method for the motion of the Moon was to punch tabular values from Brown’s *Tables*
onto cards. Since he required over half a million cards this took Comrie and his staff six
months. The cards consisted of 45 columns by 12 rows (see figure 3.1). Ten of the rows
consisted of each of the digits 0 to 9 and these numerals were printed on the cards where
the associated holes were to be punched. The top two rows were called either 11 and 12
or x and y, but this part of the column was left blank. Numbers were indicated on the
card with a single punch in each row on the digit that made it up. The x and y positions
were used to actuate certain controls on the machine. Additionally information could be
printed on the card appropriate to the task at hand, for a small fee. For example, in the
card depicted in figure 3.1 what each group of columns stands for was printed. Three
machines were used by Comrie in the work, keypunches to punch holes in the cards, the
tabulator that could sum the values on the card and the sorter that sorted cards by the
hole punched in a single column. (Comrie 1932, 697-699)
The lack of a direct subtraction feature, on the tabulator Comrie employed, led to even more cards being punched, in order to perform subtraction by addition of the 10s complement of the number. This method took advantage of the fact that when the sum of two numbers was greater than the capacity of the adding mechanism to represent it, the sum would simply lose the final carry.\footnote{For example, if the adding mechanism had a maximum number size of 99 and 75 and 30 were added to it, the result displayed would be 05. The complement of a number added to the original counter gives all zeroes. So in the case of a two digit adding machine the complement of 70 would be 30. As can be seen from the previous example, the addition of 75 and 30 in the machine gives the correct answer for the subtraction of 70 from 75.} In order to employ this method Comrie punched the complements of all the numbers to be calculated. Subtraction was also simply avoided by adding appropriate constants to some of the terms which could then be removed or nullified at a later stage. Since, finding the complements, while straightforward, would require effort comparable to simply performing the subtractions manually, I can only guess that Comrie expected to perform several subtractions per card on average. (Comrie 1932, 703-705; Bashe et al. 1986, 12)

For Comrie’s calculations the period of one of the tabular functions was subdivided
into an integer number of parts such that the needed interval (usually a day) was also evenly divisible into parts of that length. The cards, each containing the value of a function for a given time, could then be arranged so that the values of successive days occurred on successive cards in a pile. A set of piles, one for each term (function) in a harmonic series, could be assembled and organized so that by simply taking a card off the top of each pile and putting each one in the tabulator the value of the series for a day was tabulated. The value for the next day could be obtained using the cards now at the top of the piles. When the bottom of a pile was reached one simply returned to the top of the pile. In essence, each pile of cards played the role of the closed loops of numbers on Brown’s device for calculation of harmonic functions (see figure 2.1 in section 2.6). Each of Comrie’s cards was equivalent to the cardboard card in Brown’s machine setup; the cards with numbers printed on them and stuck to a loop of fabric. (Comrie 1932, 695, 701, 706)

The resemblance between Brown’s contrivance for calculating harmonic series and Comrie’s use of punched cards may just be due to the same problem being solved by both. Comrie makes no reference to Brown or anyone else’s work with calculating machines in his published report of his use punched card machines on the lunar tables. However, Comrie probably knew of Brown’s device because Brown’s work was referred to, with full citation, in a 1914 article on the Burroughs machine by the HMNAO. (Hudson 1914, 131). Comrie directly cited this article in one of his own articles on calculating machines in 1928. (Comrie 1928a, 451)

Comrie implemented procedures that helped insure that the machine had operated satisfactorily. Each card had an identified number on it and keeping track of this number, printing it and taking its sum, insured that the right cards were being used. Comrie estimated that the punched card methods: “Eliminated much fatigue, increased tenfold the speed with which results can be obtained, and reduced the cost to only a quarter of its former amount.” (Comrie 1932, 694) Comrie had calculated not only the values for
the 10 years 1935-1945, but also much of the calculations required for the period ending in the year 2000. Comrie justified this by explaining that: “There is little likelihood of Brown’s *Tables* being superseded before the end of the century.” (Comrie 1932, 706) By comparison, earlier methods to calculate the Moon’s position from Brown’s tables had required the continuous work of two people, trained in calculation, to keep up with the moon. (Comrie 1932, 694) Comrie had not only automated a gargantuan task, but as a result significantly increased the range of the analysis in doing so.

Another motivation for the undertaking was, as Comrie put it: “Also the risk of errors in copying and adding half a million figures is not inconsiderable. In the process ... the copying is done once only, the addition is done mechanically, and the results of the additions are printed.” (Comrie 1932, 697) Although punched card machines did not operate entirely error free, they reduced many of the potential sources of error. Also, the punched card format had other advantages in terms of error checking. As mentioned previously, identification numbers punched on the card helped ensure the right cards were used. (Comrie 1932, 702-703)

Whether or not Comrie was inspired by Brown’s early work on automating harmonic series calculation, Brown was well aware of Comrie’s work. Years later Comrie reported that, in the summer of 1928, Brown visited his punched card operation at HMNAO and Comrie showed him the punched card machines in operation. Comrie described Brown’s reaction as follows:

> I shall ever remember his ecstasies of rapture as he saw his figures being added at the rate of 20 or 30 a second. I think I am right in saying that the enthusiasm with which he described the process on his return first stimulated W. J. Eckert, the leading American pioneer of these machines for scientific calculations.

(Comrie 1946, 157)

Whatever the influence on Eckert, Brown was certainly enthusiastic about reporting on Comrie’s efforts. This is evidenced by a talk he gave at the American Astronomical Society’s annual meeting entitled: “Application of the Hollerith Calculating Machine to
the Lunar Tables.” The one line published abstract describes it as “An informal account of some new methods of adapting calculating machines to the computation of Ephemerides.” (Brown 1928, 596) Given Brown’s enthusiasm on the topic, it is difficult to imagine he did not communicate at least the existence of Comrie’s work to Eckert, but there remains one reason to doubt such a conclusion.

When interviewed in 1967, Eckert was unsure when exactly he first encountered Comrie’s work. He only recalled that it occurred at sometime during his doctoral studies and probably at Yale. (Eckert MS 1967a, Part I, p. 3) The only reason to doubt that it was E. W. Brown and L. J. Comrie that first introduced Eckert to punched cards is Eckert’s own report, made decades later. “My first acquaintance with automatic calculation came in 1929 when I visited the Columbia University Statistical Bureau.” (Bashe et al. 1986, 22) If Eckert meant by acquaintance physical contact then this probably was his first encounter. The statistical bureau is the second development that unfolded while Eckert was engaged in his doctoral studies.

3.3 Eckert’s Early Work with Punched Cards

In the fall of 1928, the same year that Comrie began his work with punched card machines, Ben Wood, Columbia University’s professor of Collegiate Educational Research, sent out ten letters. These letters were sent to the heads of various office machine manufacturers. Wood was looking for a way to automate the evaluation and recording of the extensive educational testing on behalf of Columbia and many other educational systems in New York and beyond. Nine of the letters received no response, but one to IBM led to a meeting with IBM President Thomas J. Watson Sr.. This in turn led to the donation of IBM punched card machines, rent free, to help Wood’s efforts. In June of 1929 this assortment of machines arrived and became the core of the Columbia Statistical Bureau. (Rodgers 1969, 135-137; Brennan 1971, 3-4) Whether or not the bureau was Eckert’s first
point of contact with the use of automated computation, it would have great significance for his career.

The machines used by the statistical bureau differed from Comrie’s in certain respects. Comrie’s machines were those of the British Tabulating Company, which had licensed Hollerith’s punch card patents for use in Great Britain. In 1928 IBM introduced the 80 column punch cards (see figure 3.2), which would remain the standard card size for IBM punched cards until they were discontinued. The Columbia bureau was also in possession of a unique machine, called variously the Columbia machine, the IBM difference tabulator, the statistical calculator and Ben Wood’s machine. The machine was remarkable for its large number of counters. Counters, also called accumulators or registers, were the part of the machine that stored the numbers. This machine had 10 separate counters and each counter had a 10 digit capacity. Punched card tabulators had multiple counters in order to keep track of subtotals and also to allow multiple different values entered on a single card to be totaled at once. For comparison, Comrie’s tabulator had 5 counters of 9 digits each. The difference tabulator was so-called because it was ideally suited to carry out the method of differences. Its special features allowed the numbers from its numerous counters to be added to each other. This transfer feature was not found in other IBM machines and was only added to British tabulators in 1932, at the request of L. J. Comrie. (Bashe et al. 1986, 22-23; Brennan 1971, 4; Comrie 1932, 697-700; Comrie 1946, 156-157) Eckert used the difference tabulator in his first recorded use of punched card machines.

In addition to the cards themselves, punched card machines had one other feature in common, the plugboard. The plugboard routed the electrical signals of the machines, like a telephone switchboard and came in two forms. The older form was the manual plugboard embedded in the machine (figure 3.3); the later form was the automatic plugboard that could be removed from the machine (figure 3.4). With the automatic version several different configurations of the plugboard could be prepared ahead of time and
then the configurations quickly changed by removing one and attaching another, which could take less than a minute. (Eckert 1940a, 13-15; Arkin)

In 1933 Eckert gave a talk to the American Astronomical Society on his first undertaking with punched card machines. By this point Eckert had become aware of Comrie’s earlier work. Also, he was soon supplying Comrie and the British Nautical Almanac with positions for the asteroid Vesta following the plan of former Astronomer Royal, Frank Dyson (1868-1939). (Eckert 1934, 9; Eckert MS no date A, 7) Eckert developed a method to calculate the orbits of asteroids (or minor planets) using what he called the method of special perturbation or Cowell’s method, i.e. numerical integration. The equations used by Eckert were as follows:

\[
\frac{\delta^2 x}{\delta t} = X = -\frac{k^2 x}{r^3} + k^2 \sum m_i \frac{x_i - x}{\rho_i^3}, \quad (3.1)
\]

\[
x = "X + (1/12)X - (1/240)X'' + (1/1951)X^IV + \ldots \quad (3.2)
\]

\[
\rho_i^2 = (x_i - x)^2 + (y_i - y)^2 + (z_i - z)^2 \quad (3.3)
\]

where \(m_i\) are the ratios of the mass of each major planet to that of the Sun, \(k\) is the gravitational coefficient for the equation, \(x\) is one Cartesian coordinates of the body being
investigated, $r$ is the body’s radial distance from the sun, $x_i$ is one Cartesian coordinate of planet $i$, one of the nine major planets, and $\rho_i$ is the distance from major planet $i$ to the body. Analogous equations to 3.1-3.3 hold for the $y$ and $z$ coordinates. The first equation (3.1) calculates the acceleration ($X$) due to the sun and major planets on the body and there are equivalent equations for the $y$ and $z$ axes ($Y$ and $Z$). The second equation (3.2) is the formula Eckert used for numerical integration taken from Cowell and Crommelin. Again "$X$ is the second summation of $X$, $X''$ is the second difference, $X^{IV}$ the fourth and so on.$^2$ (Eckert 1940a, 101-104)

The differences and such are calculated from values of the acceleration ($X$) given by the gravitational terms and then further values of position ($x$) are calculating allowing further values of acceleration to be calculated. To understand which terms are used in the formula examine figure 3.5. The values of function $X$ (for fixed time steps of the argument) are spaced every other line all in a column. The first differences are also in a

\[ x'' = X + (1/12)X'' - (1/240)X^{IV} + (1/1951)X^{VI} + \ldots \]

$x''$ being the second difference of $x$, with $x$ given by a double summation. Apparently he initially failed to note the refinement Cowell and Crommelin had added and that was emphasized by Jackson. (Eckert MS no date A)
Figure 3.4: Automatic Plugboard

(Eckert 1940a, 14)
column, the one to the right of the original values, and are on the row between the two values that generated the difference. There is a similar column for the second differences and so on. The terms in the numerical integration formula are all taken from a horizontal row in this table, all with the same time in the argument of the function. Every time a new $X$ is generated by integration, a new "$X$ can be generated one time step further along the table at a later time (and $X$'s differences can be generated further back) by addition (or subtraction) of the intervening difference terms. Thus a table of differences can resemble a pyramid with the latest values of the differences forming the diagonal edge (see figure 3.5). The values of $X$, $X''$ etc. for the same time as the new "$X$ can be extrapolated by the method of differences, by assuming the highest order difference is unchanged over the unknown interval and calculating the subsequent differences accordingly. The extrapolated value can then be used with equation 3.2 to calculate the next value $x$ at the new time and from this $x$ value new $X$ values and associated differences replacing the extrapolated values. (Eckert 1940a, 75-77, 101-104)

It is unclear how many differences Eckert actually used in his various asteroid calculations. In the equation 3.2 only differences up to the fourth are directly used, but the equation 3.2 is clearly extendable to any higher order. In later works he stated that he calculated to the fifth or sixth differences for this problem and at one point he indicated that the seventh differences of the attraction were used. However, this does not require that the sixth or seventh differences were used in the equation. Instead the sixth difference might have been calculated to ensure the error in the approximation of $X$, and other values by the method of differences, was small. In fact Eckert’s extended example seems to suggest that equation 3.2 is only ever carried out to the third term (the second difference) and higher order differences never directly enter the calculation. Possibly more terms were used when more accuracy was required. (Eckert 1940a, 52, 105, 107)

In his initial calculations for the 1933 paper Eckert had used the special Columbia statistical calculator for many of the calculations. However, he also used other machines.
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Table of $X$, argument at intervals of 1 unit, with all differences and sums.

**Figure 3.5**
He calculated the gravitational attraction terms from the sun and the planets, which involved more complex multiplications, using a Monroe desktop calculator. Punched cards were used to sum the attraction terms, calculate the differences and then accumulate them. The statistical calculator lacked the ability to multiply. By the time he presented these results in 1933 he already anticipated that all the calculations could be done using punched card machines. Specifically he suggested that the IBM’s new direct subtraction calculator and IBM 601 multiplying punch would be needed to carry out all the calculations in this way. (Eckert MS no date A, 3-4; Eckert MS 1934, 1)

The paper was well received by at least one person, Eckert’s mentor E. W. Brown. In a letter Eckert stated that: “At the meeting Professor E. W. Brown, who is the foremost authority on celestial mechanics, commented most enthusiastically on the paper.” (Eckert MS 1934, 1) At about the same time, Brown also expressed interest in the prospects for a general purpose scientific calculator. (Eckert MS 1934, 2)

As Brown and Comrie’s examples suggest, Eckert was not alone in trying to find ways to apply machines to the calculations of celestial mechanics. Many researchers attempted to find faster and more accurate ways to solve problems and take advantage of developments in calculating machines in formulating solutions to problems in celestial mechanics. Many were just attempts to choose formulas that were more tractable with hand calculators, such as the paper of R. T. A. Innes on finding the paths of comets and minor planets. (Innes 1929, 422)

Looking back, this work also fits into the tradition of Charles Babbage, who, as previously mentioned, suggested the application of automatic machine methods to astronomical calculations. Both Eckert and Comrie, like Babbage, advocated the use of the method of differences for calculation. (Cortada 1987, 61-62, 83-84) All three had been in the business of producing astronomical and mathematical tables and looked to machines for solutions to the problems involved.

---

3Innes was also an advocate of Cowell’s method as mentioned in Chapter 2.
It is clear that Comrie had some awareness of Babbage’s work. (Comrie 1946, 159; Randell 1982, 17) It has even been suggested that the example of Babbage made Comrie question the usefulness of purpose-built calculating machines versus commercial machines. A purpose-built machine means either one constructed only once or one constructed only for a very specific complex calculation, as opposed to the mass produced commercial machines that performed general arithmetic and the like. Comrie’s warnings of the potential disasters of purpose-built machines do recall Babbage to some degree, but I suspect it was Comrie’s natural sense of economy that caused him to question such enterprises more than any one example. (Swade 2000, 313; Comrie 1946, 150)

Eckert also clearly had some knowledge of Babbage given his thoughts on a new model of multiplying punch which had the transfer ability useful for the method of differences. “With slight modification one of these multipliers might become a remarkable “Difference Engine.”” (Eckert 1940a, 56) Still Eckert’s knowledge of Babbage may well have been quite limited. Like Comrie, Eckert was skeptical of purpose-built machines, Eckert once noted that a specialized machine could be constructed with standard parts, but it was unlikely to be worth the time spent and expense. (Eckert 1940a, 1) Babbage might have been the inspiration for that statement also. Whatever the influence of Babbage, Comrie and Eckert’s work would achieve Babbage’s goal of using machines to avoid the long, hard and error prone work of hand computation of astronomical and mathematical tables.

3.4 Origin and Work of the Thomas J. Watson Astronomical Computing Bureau

In 1933, Eckert convinced IBM president Thomas J. Watson to supply him with more machines in order to create a computing laboratory at the Columbia Astronomy Department with newer more flexible machines. The laboratory at the Astronomy Department began operation in 1934 with, among other things, a multiplier and subtracting tabulator.
Eckert not only obtained the machines without paying rent but, in consultation with various IBM employees, requested and obtained several modifications. (Eckert MS 1934, 2-3) In order to understand these modifications some knowledge of the machines is required.

One of the mainstays of the punched card era was the tabulator. Eckert used the Type 3-S Direct Subtraction Tabulating Accounting Machine (figure 3.6). The machine automatically adds (or subtracts as designated by an x punch) the numbers from designated portions (fields) of a card to a counter (the fields are designated by the plugboard connections). The information from the card was read from the card by a set of eighty adding brushes. The commercial version had five eight-digit counters, in essence it could store five separate 8-digit numbers. The machines used by Eckert at Columbia in the 1930s had four ten-digit counters. Each counter could add values from a single field. A device called a class selector allowed the option of one of two different fields to be read.
by the same counter as designated by a punch of the x row. The standard machine had
two class selectors. For output Eckert’s tabulator had five printer banks, one for each
counter and one that could print directly from the field of the card being read. The value
of the counters could also be visually read by the operator without printing. In addition
to the adding brushes, there was a set of eighty control brushes which allowed the
comparison of up to sixteen columns of the two successive cards as set by the plugboard.
This comparison was used to identify when groups of cards begin or end. A match of the
two cards was said to “pass control” and a mismatch called a “break control”. The break
was used to cause various results including printing totals and clearing the counters. The
plugboard also played a role in setting up the control functions. The tabulator, operating
at peak efficiency, read 9000 cards per hour while only tabulating and 4500 cards per
hour while tabulating and printing the values on the card. (Eckert 1940a, 12-16)

A device often used with the tabulator was the Automatic Summary Punch. The
summary punch was a device connected to the tabulator by an electrical wire that allowed
the sums, tabulated in the counters, to be punched into cards, useful when the sum was
required in subsequent calculations. The summary punch could also change the sign of
numbers from a given counter with the flick of a switch. Also the summary punch had a
plugboard that allowed it to select the digits from the counters to be punched. (Eckert
1940a, 16-17).

The most complex calculating machines used by Eckert in the 1930s was the IBM
601 Automatic Multiplying Punch (figure 3.7). The multiplier read and multiplied two
numbers from designated fields on a card and punched the result on the card in a different
field. The multiplier also calculated the sum of the products in a given group that could
be read off. The mechanism consisted of a ten-digit counter for the sum of products
and six eight-digit counters. The multiplicand and the multiplier were each read into a
separate eight-digit counter. The other four counters were divided into a pair of counters,
connected in series, creating, in effect, two sixteen-digit counters. Multiplication was
carried out by multiplying each digit in the multiplicand by each digit in the multiplier one at a time in an electric multiplication table. The two sixteen-digit counters were used to sum the partial products that were calculated. One sixteen-digit counter summed the higher order digit of the partial product and the other counter accumulated the lower order digit of the partial products. Finally, one sixteen-digit counter added into the other, the result was punched on the card and added to the sum of products. An additional feature of the multiplier was crossfooting which allowed one or two numbers to be added to or subtracted from the product of the multiplication by reading into the sixteen-digit counters from the card. Rather than a complete set of eighty control brushes the multiplier had four or five movable control brushes which detected the presence or absence of a punch in the x position. The multiplier could be set to switch the multiplier and multiplicand, not to punch the result and only accumulate the sum of products in order to check the multiplication of a group of cards, either simply through comparing the sum of products or, in later models stopping when the product punched did not match the product tabulated. The multiplier was one of the slowest of the automatic machines and operated at around 730 cards an hour for eight-digit by eight-digit multiplication but at about 1350 per hour for three by eight. (Eckert 1940a, 17-20, 28) As a result, in some cases, multiplication by repeated addition (progressive digiting or totaling) could be faster than the use of the multiplier punch. (Bashe et al. 1986, 22)

In addition to simple arithmetic machines, punched card machines existed that could physically sort cards according to the value punched (figure 3.7). The Counting Sorter used a single movable sorting brush to monitor a single column to be sorted on (figure 3.8 and 3.9). The sorter had thirteen receptacles into which the cards could be sorted, one for each punch position plus a reject receptacle. Any of those receptacles could be closed off so that ones which would normally go there were instead placed in the reject group. The sorter had a counter attachment which calculated the number of cards in each receptacle, the number in the group and the grand total for all groups. The machine could both
Figure 3.7. IBM 601, Multiplying Punch

(Baehne 1935, 15)
sort and count or only sort or only count. It could operate at the rate of 20,000 cards an hour making it perhaps the fastest punched card machine. (Eckert 1940a, 10-12).

The High Speed Reproducer (or Automatic Reproducing Punch) allowed the fast copying of cards. These machines could operate in either a standard or gang punching mode. In standard mode the cards to be copied and the blank cards to be punched operated in separate card feeding devices. The cards to be copied would pass through two sets of brushes, while the blanks passed through a set of punching knives and then a set of brushes. The first set of brushes the originals passed through detected the hole and actuated the set of punching knives in the parallel card feed. After the holes had been punched the originals and copies would each pass through a set of brushes and a comparison would be made to ensure that the holes in the original and copy matched. If a mismatch was detected, the machine stopped. In gang punching mode a single master
Figure 3.9 Close-up of the sorter’s card reader.

(Baehne 1935, 10)
card was passed through the punching feed, it was left uncut but the comparing brushes
would actuate the punching knives to punch the blank card placed behind it. The newly
cut copy’s holes would then pass through the brush controlling punch of the card behind
it and so on. A new master card could be indicated by an appropriate x punch, which
would prevent the knives from operating on the card. (Eckert 1940a, 20-21)

The other machines described in detail by Eckert include the motor-driven electric
duplicating key punch, used to punch the holes in the cards controlled by use of a key-
board, or duplicating a pre-punched card placed in the machine. The Verifier was a
machine used to check the cards had the right punching. If the number punched on the
card was pressed on the keyboard of the verifier then the card was moved to the next
column and if not the card stops. The Interpreter printed, at the top of the card, the
number punched on the card. The version used by Eckert could type the values from as
many as 45 of columns selected by a plugboard. (Eckert 1940a, 8-10, 21)

As noted, Eckert’s machines were slightly modified from the normal commercially
available IBM machines. The most important modification was the creation of the cal-
culation control switch that allowed him to orchestrate the complex calculations of the	tabulator, multiplier and summary punch (figure 3.10). In the process of numerical inte-
gration carried out by Eckert, the control switch went through a periodic series of twelve
steps, each with different configurations for the multiplier, tabulator and summary punch.
The switch could change the plugging of the machines and also toggle various switches
on the machines. (Eckert 1940a, 22) The calculation control switch is often noted as a
significant step in the development of automatic control. (McPherson 1984, xii)

As revealed by a diagram in Eckert’s papers and described in various publications, the
calculation control switch consisted of two main parts, a set of twenty rotating notched
disks which opened and closed circuits and a relay or switch box. (Eckert MS 1970;
Eckert 1940a, 77) This was connected to the multiplier, summary punch and tabulator.
He described the switch box in one publication: “The wiring is done through a special
switch box consisting of 6 electrically operated twelve-pole double-throw gang switches.” (Eckert 1935b, 180) Confusingly, in publications he would only describe the switch box or the notched disk part of the mechanism. When he first proposed what would become the Control Switch to IBM in 1934, it was only this switch box that is mentioned. Also, he suggested taking the box from the Difference Tabulator, which is apparently what happened. However, in his 1934 proposal, he suggested the switch box itself would be controlled via a series of external switches and not the set of notched disks. It would appear the notched disks were made by IBM to order for the project. (Bashe et al. 1986, 23; Eckert MS 1934; Brennan 1971, 8; Cortada 1987, 84) Although simple compared to later machines, the punched card accounting machines were still complex apparatuses. The calculation control switch hinted at the possibility of full automation of calculation and of its utility for scientific calculations.

The calculation control switch was designed to partially automate the numerical integration procedure Eckert had developed (see equations 3.1-3.3 in section 3.3). The first part of the calculation to be done was the effect on the acceleration ($X$) due to the major planets by using an approximate value for the positions of the asteroid. The planets made a relatively small contribution to the acceleration and so their positions and masses needed only an approximate determination. Cards were grouped together by date and there was one card for each position of each major planet ($x_i$) and a card for the position of the asteroid ($x$). The tabulator and summary punch were used to find the relative coordinates ($x - x_i$). The square of the radial distance ($\rho^2_i$) between each planet and asteroid was then calculated by the multiplier punch. Rather than find the square root of this value a table was used, with interpolation, to find the necessary values for equation 3.1. The remaining constants were multiplied in and the terms, due to all the planets, were summed, giving a set of cards with the net perturbations due to the major planets as a function of time. (Eckert 1940a, 102-104; Eckert 1935b, 178-179) Thus an entire set of computations would be done before the main integration.
Punched card machine setup for the integration of differential equations. Left to right: tabulator, summary punch, calculation control switch, and multiplying punch. (IBM Archives)

**Figure 3.10**

(McPherson 1984, xvii)
The process of numerical integration was one of Eckert’s most carefully orchestrated uses of punched cards. The final step was the calculation of the latest set of differences and summations from the newly calculated X value (also the equivalent Y and Z values were calculated). The tabulator was used to calculate the new differences by repeated subtraction. (Eckert 1940a, 50-53)

Now supplied with the new set of differences, the Operator would set the Calculation Control Switch to position 1. An estimate for X at the new time was found by assuming the highest order difference remains constant, adding the current X and all the differences on the diagonal of latest differences. This sum was done by putting the relevant cards in the tabulator and supplying cards to the summary punch, three cards were produced one each for estimates of X, Y and Z as the switch goes through positions 2, 3, and 4. The formula for numerical integration (equation 3.2) was used to generate values of position (x, y and z) based on the estimated acceleration values and differences. (Eckert 1940a, 108-109)

The estimated values of the position were copied onto four cards, while the switch was moved through its 5, 6 and 7 position. The operator generated a value for \( r^2 \) shown on the multiplier from the first three cards. The value actually required for computation was \( 1/r^3 \). A table relating \( r^2 \) and \( 1/r^3 \) was punched onto a set of cards and the operator copied the nearest entry of this table onto the card along with the last four digits of \( r^2 \). The operator moved the switch to position 8 and performed a linear interpolation\(^4\) on the multiplier to find \( 1/r^3 \). (Eckert 1940a, 107-109)

Finally the operator multiplied the value of \( 1/r^3 \) by the gravitational coefficient (\( k^2 \)) and the values of x, y and z, with the control switch in positions 9 and 10 respectively. At the same time the operator calculated the value of the highest order differences again on the tabulator. The operator moved the switch to 11 and added the acceleration due to the planets to the solar acceleration, giving a new value for X. The operator then compared

\[^4\text{A linear interpolation is of the form } y = y_0 + x(y').\]
the values of the estimates of X and its differences with the new value of X to ensure that the error (highest order difference) was small. Finally the operator tabulated all the differences and summations based on the new value of X, thus completing one more cycle. (Eckert 1940a, 104-110) The calculation control switch allowed all these steps to be carried out in sequence without having to change plugboards or even toggle switches (simply press one button). This was vital because each step of numerical integration had to be carried out before the next step was executed. One could not gain economies of scale by doing all the calculations of a certain type in one setup before changing for the next set of calculations. Despite the fact that numerical integration is called a brute force technique, Eckert’s approach shows that subtlety and innovation were required to tackle the problem with the machinery of the day.

The calculation control switch is often noted as being an early example of automatic calculation. (Campbell-Kelly 1990, 148-149) However, as the example shows, a great deal of operator intervention was still required. Although, as mentioned in Chapter 2, there were many advocates of Cowell’s method before Eckert began using it, many felt that Eckert’s adaptation of the method to punched card machines made it far more widely applicable. (Herget 1946, 115) Compared to Cowell’s original application, Eckert avoided the expedient of varying the time step and, unlike Numerov, he did not feel the need to search out a set of coordinates for easier computation. Faster easier computation of the method meant that Eckert could apply numerical integrations at shorter time steps for more precision and accuracy and over a longer period thus extending the range of prediction.

In addition to these main considerations Eckert’s work with numerical integration had other advantages. The reliability of punched card machines contributed greatly to their success in scientific calculation. Errors early in the numerical integration would have had disastrous results. Another advantage to the use of punched card machines was the ability to reuse the same set-up for further problems and easily reproduce the cards,
once made, required for a computation. As when Comrie found it easy to extend his
calculations with Brown’s * Tables once the initial cost of punching the set of cards was
done, Eckert found a similar economy. In this case it was the punching of the positions of
the major planets and the set-up of the machines which was initially expensive. However
he pointed out that, once done, the positions of the major planets could be used for
many subsequent problems and the set-up was also reusable for a variety of problems.
In addition the ease of copying meant that he could easily supply sets of cards to others.
(Eckert 1935b, 181; Eckert 1940b, 102)

In 1937 Eckert published a work on correcting calculated orbits with observational
data with Dirk Brouwer (1902-1966), this would remain the standard method with only
slight modification for much of the rest of the century. The basic concept of the differ-
etial correction of orbits is like that of other curve fitting, to create a set of equations
of condition in which can be used to generate a best fit to the data. In this case the
data was observations and the fitting occurs from the numerical integration in rectan-
gular coordinates. One novelty of this method was the use of rectangular coordinates
instead of the spherical polar coordinates of observation. The corrections derived from
the procedure could be used to generate a new set of initial conditions to recalculate the
numerical integration or to improve the elements of the Kepler ellipse used to describe
the orbit. (Eckert and Brouwer 1937, 125, 128; Marsden 1995, 190)

Eckert employed the machines in calculating the orbits of many asteroids. It is dif-
ficult to gauge how many orbits were calculated over the course of the 1930s, Eckert
never published the results in articles under his own name. One significant project that
relied on Eckert’s input was a set of 16 asteroid orbits done for Dirk Brouwer at Yale.
These calculations were done in order to create more accurate reference points in making
measurements of celestial position. In other cases other researchers used the machines
for asteroid orbits and published their work. (Eckert 1959, 149-150; Hertz 1938, 36)

Perhaps the most extensive numerical integrations performed on the Watson Bureau’s
punched card machines were integrations of the orbits of Uranus and Neptune, around 1940. These computations were performed under the direction of Dirk Brouwer at the Yale Observatory. In addition to information on the orbits of those two planets, further calculations were made to attempt to gauge the influence of Pluto on the other two planets. This in turn allowed an estimate of the mass of Pluto. The full results of these calculations were never published and it is not clear to what extent the calculations were completed. They would play a role in the numerical integration of the outer planets which will be discussed in Chapter 5 of this thesis. (Brouwer 1940, 7-8; Russell 1940, 18)

In 1937 the punched card resources at Columbia were reorganized as the Astronomical Hollerith-Computing Bureau. Within a few months of its inception the Bureau was renamed the Thomas J. Watson Computing Bureau. (Brennan 1971, 8-9) The bureau was organized with the cooperation of the American Astronomical Society, IBM and the Department of Astronomy of Columbia University. The intent of the bureau was explained as follows: “The Astronomical Hollerith-Computing Bureau is to operate as a scientific non-profit-making enterprise under a board of managers.” (Eckert 1937, 249) The board of managers was made up of five members, three appointed by an Advisory council of the Astronomical Society, the other two members were appointed, one by IBM and the other by the Department of Astronomy of Columbia University. (Eckert 1937, 250) Eckert was one of those five board members. (Eckert 1940a, ii) The hope was to make the machines and techniques of the bureau more widely available. A sense of both the ambition and limitations of the vision of Eckert and his associates is suggested by Eckert’s statement fifteen years later that: “At the time it was anticipated that the Bureau would be the national and probably the international center for such work and that few, if any other punched card installations, would be feasible or necessary.” (Eckert 1951) The bureau would never become the imagined center for computation but this was in part because of the unanticipated explosion in computation to come.

The bureau continued the numerical integration of orbits and other work that had
begun in Eckert’s computation lab. All the work carried out by Eckert and the bureau during the 1930s would be summarized by Eckert’s 1940 book *Punched Card Methods in Scientific Computation*. Published by the bureau, the book was to become known as “the Orange Book” because of the bright orange cover of the publication. (Brennan 1971, 9) Herbert R. J. Grosch (1918-2010), one-time colleague of Eckert, student of celestial mechanics and early computer industry professional, referred to it as Eckert’s “Orange Opus.” Grosch claimed to be the only person who had to buy it, as many copies were given away to interested scientists by IBM. (Grosch 1991, 59; Rodgers 1969, 142) In this book Eckert detailed his work in lunar and planetary theory with punched cards as well as his extensive repertoire of computational techniques with punched card machines. It had an influence on many in the nascent field of computer science. For Grosch it was his “second sign post” on the computer road. (Grosch 1991, 58)

The book was also well received by reviewers. One anonymous reviewer said prophetically “The book ... is probably the first of this kind to be published.” (Book Review 1941, 112) Some would later view Eckert’s book as the first computer book. Comrie also wrote a favourable review, he had only one complaint, Eckert’s failure to discuss costs. “[H]is schemes appear at times to savour of extravagance.” (Comrie 1941, 131) As this statement indicated, Comrie was less liberal in using expensive punched card machines. The free use of the machines by IBM undoubtedly encouraged Eckert in his use of them. The book is one of the key records of the work that Eckert did in the 1930s with punched card machines. It is also a testament to the complexity of applying the limited punched card machines of the era to general problems.

The other major research in celestial mechanics that Eckert undertook was to verify and slightly improve E. W. Brown’s lunar theory. This was done with Brown’s help and at his request. Part of Brown’s impetus and his most direct contribution to the project was to suggest the coordinate system used in the calculations. In his original derivation, Brown had adopted a coordinate system, used by Hill, that tied the system
to the mean motion of the sun. In this new verification project, coordinates were linked to the mean motion of the moon, a modification of a scheme originally suggested by Euler in his second lunar theory. Also, Eckert and Brown felt that some coefficients, including those parts of the theory giving the motions of the Moon’s perigee and node, were not yet sufficiently accurate and sought to improve them. In addition to the aid of the Watson Computing Bureau, this project received a grant from the Penrose Fund of the American Philosophical Society that covered various expenses including the cards used in the operation. The bulk of the work seems to have been done in the period between 1936-1938. (Brouwer 1939, 305; Brown 1938, 785-788; Eckert 1940a, 97-98; Eckert MS 1943, 3-4)

Brown’s theory involved the algebraic solution of the differential equations of motion with the numerical values inserted at the end. Therefore verification using Brown’s method would require redoing the calculations which had taken Brown twenty years and improvement would require starting again from the ground up with a new set of equations. Instead Eckert and Brown employed Airy’s method. As discussed in Chapter 2, Airy’s method is a numerical solution in which the previous best solution was substituted into the equations of motion (see equations 2.1-2.3). This allowed previous results to be built upon to create a new set of equations of solution. The value of the numerical technique was dependent on the high accuracy of the solution used. The results of the substitution were residuals or errors indicating the level of precision achieved with the original equations, which provided a means to improve the precision and hopefully the physical accuracy. A new more precise theory could be obtained by using the residuals to calculate a set of equations of variation (equations of best fit) giving final corrections. Note that the initial solution used for this procedure was the fundamental theory of Brown in rectangular coordinates of the main problem of lunar theory and not the tables that contained approximations, the perturbations of the planets and used spherical polar coordinates relative to the Earth. (Eckert 1940a, 97-99; Eckert MS 1943, 30-35)
The equations of motion used for substitution were modified from the ones given by Airy (2.1-2.3) and appeared as follows:

\[
x \frac{dy}{dt} - y \frac{x}{t} + (x^2 + y^2) = \int \left( \frac{x \delta \Omega}{\delta y} - y \frac{\delta \Omega}{\delta x} \right) dt + C_1
\]  

(3.4)

\[
\frac{1}{2} \frac{d^2}{dt^2} (r^2) + \frac{1}{2} (x^2 + y^2) - \frac{1}{2} \left[ \left( \frac{dx}{dt} \right)^2 + \left( \frac{dy}{dt} \right)^2 + \left( \frac{dz}{dt} \right)^2 \right] = \\
\Omega + \left[ r \frac{\delta \Omega}{\delta r} \right] + \int \left[ 2 \left( x \frac{\delta \Omega}{\delta y} - y \frac{\delta \Omega}{\delta x} - \frac{1}{n} \frac{\delta \Omega}{\delta t} \right) \right] dt + C_2
\]  

(3.5)

\[
x \frac{d^2 z}{dt^2} + z \left[ - \frac{d^2 z}{dt^2} + x + 2 \frac{dy}{dt} + \frac{\delta \Omega}{\delta x} - \frac{x}{z} \frac{\delta \Omega}{\delta z} \right] = 0,
\]  

(3.6)

where \( x = 1 + x_1, x_1, y, z \) are the deviations of the Moon from circular motion in the plane of orbit (the x-y plane) and \( t \) is time. These coordinates are derived from Brown’s theory. \( \Omega \) is defined as the portion of the force function such that its partial derivatives with respect to \( x, y \) and \( z \) are the acceleration of the Earth and Moon due to the action of the Sun. Omega is further decomposed as a series: \( \Omega = \sum_j \sum_h k_{jh} \omega_{jh} \), where \( k_{jh} \) contains successive powers of a small constant and each \( \omega_{jh} \) is the product of homogeneous function of \( x, y, z \) of order \( j \) by a harmonic series with argument \( i \) and with known numerical coefficients. The \( \Omega \) series coefficients are defined by numerical values of the parameters, the best available empirical values. The \( x, y \) and \( z \) coordinates refer to the centre of the Earth as the origin, but rotating about the z-axis with the mean angular velocity of the Moon. The x-y plane is taken to be the plane of the Sun’s orbit about the Earth. (Eckert 1940a, 99; Eckert and Smith 1976, 201)

Since the solution used for substitution was Brown’s, which consisted of a complex harmonic series of trigonometric terms, the calculations were very protracted involving the multiplication and addition of series of trigonometric functions. Note that Eckert delineated the form of the solution for \( x, y \) and \( z \) as follows:

\[
x = \sum a_i \cos \alpha_i
\]

\[
y = \sum b_i \sin \alpha_i
\]

\[
z = \sum c_i \sin \alpha_i
\]
\[ \alpha_i = (jp + kq + lr + ms) t \]

\[ j, k, l, m = 0, \pm 1, \pm 2 \ldots \]

\[ p, q, r, s = \text{given numbers} \]

\( a_i, b_i \) and \( c_i \) are the coefficients of the solution, implicitly as in Airy's original method a residual term (to be solved for) is added to each. The numerical value of the derivative for each term is the coefficient of \( t \), \((jp+kq+lr+ms)\). Eckert explains the derivatives are calculated by multiplying each term by the coefficient of \( t \) and changing the sign where the terms contain a cosine. (Eckert 1940a, 99)

Although Comrie had dealt with harmonic series in his work, the goal here was not a numerical evaluation of the final sum but the numerical coefficients and arguments for a new combined series. This was algebraic bookkeeping by machine. In order to multiply the series the following trigonometric identities were used:

\[
2\sin(x)\sin(y) = \cos(x - y) - \cos(x + y)
\]

\[
2\sin(x)\cos(y) = \sin(x - y) + \sin(x + y)
\]

\[
2\cos(x)\cos(y) = \cos(x - y) + \cos(x + y)
\]

\[
e^x e^y = e^{xy}
\]

\[
2\cos(x) = e^{ix} + e^{-ix}
\]

\[
2i\sin(x) = e^{ix} - e^{-ix}
\]

First Eckert would multiply the coefficients in front of the trigonometric functions \((a_i, b_i, \text{etc.})\) with each other rejecting (leaving out) those products (and associated terms) whose result would be too small (less than \(10^{-10}\)). The cards were sorted by the value of the coefficients to make the rejection process quicker. Then the sum and difference of the arguments (actually the coefficients of time) were calculated for the sum that replaced the multiplied set of series. Since the arguments were (usually) a limited number of integers,
the sums and differences were found by the sorter and punched from a set of master cards containing all possible values using the high-speed reproducer. The arguments and coefficients were organized on the cards so that the product was represented by a new trigonometric series with the coefficient of each term and the argument of the trigonometric cards prepared on it. (Eckert 1940a, 65-74) The resulting series could then be evaluated like any other series as needed.

In this way Eckert was able to do most of the work necessary to verify Brown’s theory in two years. It is unclear if any work occurred to derive a corrected solution from the residuals. A manuscript detailing the results was in preparation by at least 1943. However, the work would not be published, because of Eckert’s other commitments. (Eckert MS 1943, title, p. 2 verso; Eckert 1940a, 98) In the 1950s he would pick up and extend the work significantly as discussed in Chapter 6 of this thesis.

As discussed in Chapter 2, Airy had been unable to implement his lunar theory due to the mistaken inclusion of a non-existent term. Eckert’s work has been lauded for vindicating the work of his teacher Brown, showing that his theory and the Tables were without a flaw. (Rodgers 1969, 142) On the other hand, few mention how his work vindicated the hard work of Airy’s final years. Airy, announcing the end of his attempt at numerical lunar theory, remarked: “I believe the plan of action which I had taken would, if properly used, have led to a comparatively easy process, and might in that respect be considered as not destitute of all value.” (Airy 1888b, 2) The work Eckert did on lunar theory, with punched card machines, showed the machine’s potential to speed up complex computation and allow powerful approaches to theoretical work in physics to be implemented that would not otherwise have been possible.

Eckert also worked on a few other astronomical problems with punched cards. He worked on planetary theory in what he called the computation of general perturbation, the solution of the equations of planetary motions in terms of harmonic series, and finding equations for the orbits of (minor) planets in much the way he had for Hector
in his Phd thesis. Here Eckert applied the same punched card methods for series that he used in dealing with Brown’s theory. In particular, Eckert attempted to work out a method that had been worked out by Brouwer and Brown. It started from a potential function that included logarithmic functions as terms. Although Eckert prepared a set of cards with the needed logarithms to allow quick calculation, he did not find enough need to mechanize the finding of antilogarithms, to convert back from logarithms to natural numbers. (Eckert 1940a, 112-114) The computation of new orbits would also require synthesis of a Fourier series.

In this vein Eckert devoted a chapter of his book to the synthesis and analysis of harmonic series. Synthesis is what Comrie had used to prepare his lunar table calculation by punched card machine. It consisted in finding the sum of a series. Analysis starts with the values of the sum for specific values of the argument and determines the coefficients of the trigonometric functions in the series. Eckert demonstrated how this can be done mechanically, by adding an appropriate series to derive the value of the coefficients. (Eckert 1940a, 57-64) Eckert concluded a discussion of punched card methods in planetary theory by saying, “Thus the elaborate and extensive calculations involved in planetary theory may be done almost entirely without hand work.” (Eckert 1940a, 114) In this instance much of the algebraic work remained the same, but the solution could be found more easily with punched cards.

Another major project at the Watson Astronomical Computing Bureau was the production of star catalogues. Standard star catalogues at this time were derived from a set of photographic plates. The positions on the plates had to be from the rectangular coordinates of the plate to the spherical coordinates of the sky. Also comparison with older catalogues necessitated the calculation of the precession of the stars over time. These were the calculations for which the Bureau’s punched card machines were used in the case of the Yale Zone Catalogue (Eckert 1940a, 79-89). A similar problem occurred in a stellar photometry project carried out by the Rutherford Observatory at Columbia, where the
magnitude of the stars were determined from photographs using a galvanometer. It was necessary to perform a set of calculations to establish the true magnitude to account for variations in the photograph and their background light levels. (Eckert 1940a, 90-96) Again punched card methods were used to expedite the process of calculation.

While these problems may seem trivial, Eckert felt, in the case of the stellar photometry survey, that the punched card methods allowed the project to be significantly extended. (Eckert 1940a, 90) Equally important, the punched card form of the catalogues allowed for easy sorting, calculation of statistics and copying for sharing the resource with others. In one case Eckert and the workers in his lab found it useful to encode the entire Boss star catalogue onto punched cards. (Eckert 1937, 253; Eckert 1940b, 376-378)

His work with these star catalogues shows how Eckert was also applying his talents and machines to more routine problems. Also, these endeavours showed that the speed of calculation is not the only consideration in using punched card machines.

It should be noted that Eckert did not in general operate the IBM machines himself. In fact, most of these computations had been personally performed by Lillian Feinstein, a worker in the bureau, who also made key contributions to the development of the procedures. (Eckert 1940a, iv)

### 3.5 Dr. Eckert Goes to Washington

In December of 1939, a letter from the Columbia president informed Eckert that he would receive a promotion to full professor at Columbia and would receive a $6500 per annum salary. (Butler MS 1939) However, in 1939 the office of Director of the Naval Almanac at the U.S. Naval Observatory in Washington had become vacant. A search began for the new director. Eckert’s Yale colleague, Dirk Brouwer, was the first person offered the position. Brouwer refused and recommended Eckert as the candidate. The search continued and in September of 1939 Eckert submitted his own application to the US Civil
Service Commission. Given the strategic thought Eckert put into his choice of references, he was not certain he would be chosen for the position. (Eckert MS no date D) In fact he was the favoured candidate by a considerable margin judging from the letters he received imploring him to accept the position from J. F. Hellweg, Superintendent of the Naval Observatory. The extent of Hellweg’s enthusiasm for Eckert was demonstrated by such statements as: “I have never wished for anything so hard in my life as that you will accept the position and come to Washington.” (Hellweg MS 1939) The looming possibility of the United States entry into the Second World War and the strategic importance of the NAO for US forces must have contributed to the urgency of Hellweg’s request. Eckert himself admits that Hellweg’s intense interest played an important role (Dick 2002, 519)

Interestingly Grosch indicates that, for Eckert, astronomical research, specifically celestial mechanics, was always primary and that other issues of computation were of secondary importance. In Grosch’s estimation, Eckert’s work at the Almanac Office was undertaken with some reluctance. Brennan on the other hand intimates that Eckert enjoyed the resources and challenges of the work at the Almanac Office. Eckert’s acceptance letter to Hellweg confirms that the prospect for scientific work was a significant motivation in accepting the offer. (Brennan 1971, 10-11; Grosch MS 2003) Eckert’s correspondence at the time indicated he was conflicted. He commented to Hellweg that: “One does not fully appreciate a university atmosphere until he is faced with the immediate prospect of leaving it.” (Eckert MS 1939a) In September of 1939, when he applied for the position, he admitted to one of his references he had not yet decided if he would take the position. “If I am offered the place the decision will be one of the most difficult I have had to make.” (Eckert MS no date E)

Eckert suggests that financially the move was something of a sacrifice. On the other hand the per annum wages were equal to that he would have received with his promotion to full professorship, $ 6500. Indeed in correspondence with the Columbia University president, Nicholas Butler, he commented on the full professorship: “It has made it pos-
sible to make a difficult decision on the basis scientific opportunity and public service rather than of economic necessity.” (Eckert MS 1939b) Outside of his own salary, Eckert was also concerned about the problems of selling his house and moving his family. Whatever his exact reasons, or regrets, Eckert began his work at the Naval Observatory’s Nautical Almanac Office on February 1st, 1940. (Eckert MS 1939a; Eckert MS 1939c; Dickey MS 1940)

The punched card Bureau at Columbia continued its work during the war under Jan Schilt (1894-1982) of the Columbia Astronomy department, who would often consult with Eckert via mail, while Lillian Feinstein continued managing the machine operations. Eventually, the Bureau was assigned war work for the US government in fields such as ballistics and this led to large expansion in its complement of machines. (Grosch 1991, 28; Brennan 1971, 10-11) Similarly Eckert continued to work closely with IBM in this period, visiting IBM to consult about special modifications required for the Nautical Almanac machines. Eckert also gave a presentation in one of IBM’s courses for its technical employees in 1944. (Eckert MS 1944, 1-2)

Eckert’s contribution was both to automate the calculations of the Almanac office with IBM machines (when he first arrived all work was done by hand or with desk machines, none of which could print) and, more importantly, produce a new Air Almanac to aid U.S. airplane pilots in navigation. (Brennan 1971, 10) These air and nautical almanacs gave positions of the Sun, Moon, planets and stars. These tables allowed the navigator of a ship or plane to determine his position by measuring the position of some heavenly body. Comparison of the position of the heavenly body with the table would allow the determination of local time. Local time could be compared with a clock set to some standard time of known longitude (such as Greenwich Mean Time) and used to calculate the longitude of the observer. The innovation of the Air Almanac was to create a much closer spacing in time of the tabular data, thus the observed angle would be closer to one of the values given in the tables and simpler interpolation schemes could be used to
make the comparison. (Eckert 1944, 12-13)

Therefore much of the calculations mechanized by Eckert was of the sort done by Comrie on Brown’s *Tables*, but this was on a much larger scale. Eckert’s equipment at the Office was eventually quite extensive and included a tabulator with a counter that used sexagesimal digits for handling hours, degrees, minutes, seconds and the like. (Brennan 1971, 10) At first he had limited resources, restricting his rental of IBM machines to only part of the year and needed to borrow from Comrie’s other work and make careful use of desk machines to calculate values for the NAO’s tables. Eckert found that he needed not only machines, but also trained people to carry out the conversion of the Almanac Office to automation. He hired Lillian Feinstein to help train the workers and carry out the calculations in the summer of 1940. (Report *MS* 1941; Grier 2005, 242) At Columbia Eckert had created a uniquely qualified set of personnel in addition to his one-of-a-kind equipment.

The technical innovations of this project though were the efforts to remove error and ensure clarity of the information. Eckert would describe the method used to produce the *American Air Almanac* as one where “the human element has been almost entirely eliminated.” (Eckert 1944, 15) The printer’s proof was checked by having the numbers on it punched onto cards. The resultant cards were checked against the original cards by machine and the columns of figures were differenced as a final check. This practice began at least by the end of 1940. In later years the master copies of the almanac were prepared directly by punched card machine and photographically reproduced. Eckert later wrote: “The efficiency and accuracy of this method are revolutionary.” (Eckert 1944, 15). It is sometimes claimed that not a single error has been found in the Almanacs printed by this method. (Grosch 1991, 62; Eckert *MS* 1940b) In this work, more than any other, accuracy was paramount because of its work guiding US military planes.

The actual printing of the tables was also a revolution as the standard punched card machines could not print the results at the size or level of clarity that was required. At
first a jury-rigged solution, that required constant operator intervention, was employed. It involved replacing the printing stamps in a standard IBM tabulator with higher quality better spaced ones, however in this way only half a line could be printed at a time. The layout was the work of one Jack Belzer, an observatory supervisor. A sense of how dedicated Eckert was to details is his consultation, with Bell telephone typographers, on the best font for use in a small type reference work, like the *Air Almanac*. Bell had developed a Bell Gothic font for use in their phonebooks. After the war a special printing machine, an IBM typewriter controlled by punched card, was supplied. Eckert was quite proud of this machine for its versatility and elegance. (Head Employment Branch *MS* 1944; Eckert and Haupt 1947, 201; Grosch 63-65)

Of all Eckert’s work, his work at the NAO would have the greatest immediate practical importance. The increased detail of the almanac, allowed by automation, shrunk the time required for a navigator to find a position from 30 minutes to as little as 1 minute. The accuracy was vital to ensure safe air navigation. Some suggest the quick navigation his Air Almanac allowed was key for anti-submarine warfare to save Allied shipping, because it allowed a quick response to submarine attacks. A similar claim was made by Paul Herget (1908-1981), an astronomer who worked with Eckert at the NAO, for a table of spherical trigonometric functions used by the US navy to triangulate the position of German U-boats from their radio broadcasts picked up by various long-range listening posts. Herget calculated the table with the aid of the NAO machines and two Navy personnel at night over the course of three months in 1943. The end of 1943 saw a large drop in the losses of Allied ships to U-boat attacks. (Brennan 1971, 10; Grosch 1991, 62; Rodgers 1969, 143-144; Dick 2002, 522-523)

However, these claims to the efficacy of the NAO’s efforts in thwarting the U-boat campaign should be put in perspective. The Allied forces employed a wide variety of countermeasures from sonar and radar to improved evasion tactics in attempting to reduce shipping loses. Over the course of the Second World War, losses due to U-boat
varied widely from month to month for a variety of reasons and so the impact of navigation
tables like the *Air Almanac* cannot be gauged. The introduction of microwave radar on
Allied aircraft is usually stated to be the main cause of the precipitous decline in Allied
shipping losses in 1943. Still, navigation remained an important strategic capability as
suggested by the effort to set up the LORAN radio navigation system during the war.
Similarly high-frequency direction finders (HF/DF) for radio were an important means
for finding submarines before and after the inception of radar. The Allied effort against
submarines relied on many redundant efforts for its efficacy. (Morison 1963, 118, 122-128;
Dick 2002, 523) In any case, Eckert’s efforts at the Almanac office showed his ability to
organize and the value of his punched card techniques.

During the war Eckert published an article on Air Almanacs. It was the December
1944 edition of *Sky and Telescope*, an astronomy periodical for a wide audience not just
academic specialists. The publication suggests Eckert’s pride in his and his coworkers
accomplishments at the Almanac Office and Eckert’s attempts to communicate his de-
velopments in punched card machinery. (Eckert 1944) This is the first example of him
publishing on punch card methods in a popular forum, but his previous articles, and
especially his book, emphasized the many possible uses of punched card machines for sci-
ence. Whatever reservations he had about work outside theoretical celestial mechanics,
Eckert clearly realized the potential of punched card machines and felt a responsibility
to publicize their utility and apply his skills with them.

3.6 **Impact of Eckert’s Work with Punched Card Ma-
nichines**

Eckert’s association with IBM in the 1930s marked an earlier milestone in terms of
collaboration of IBM with research scientists. However, he was not alone. In addition
to Comrie’s and Ben Wood’s efforts, other researchers employed punched card machines
for statistical analysis and other work. The most visible laboratory engaged in such research, other than Eckert’s, was at work at Iowa State University. George Snedecor (1881-1974), a mathematics professor, organized a lab in 1927 as the “Mathematical and Statistical Service.” Years later John Vincent Atanasoff (1903-1995), an Iowa State physics professor, would employ the machines, slightly modified, at the University to solve a problem in spectroscopy and published a paper on this in 1936. Atanasoff would lose access to the laboratory machines and go on to design a device for the solution of simultaneous equations that would employ vacuum tubes and binary arithmetic. In 1973 US courts would declare this machine as the source of key concepts of the electronic digital computer and invalidating patents associated with the ENIAC. (Atanasoff and Brandt 1936, 83; Grier 2000, 54-57; Cortada 1987, 14)

At the end of the 1930s Linus Pauling’s (1901-1994) x-ray crystal structure group at the California Institute of Technology established a punched card lab, with the assistance of Eckert and the Bureau’s work. Originally Pauling’s group had planned to build a specialized purpose-built machine for the Fourier analysis of crystal structure at an estimated cost of $7500. Eckert and the bureau were able to convince them that an IBM tabulator, with some minor modifications, would be adequate to carry out the analysis. The tabulator, and associated techniques would be used by Pauling’s lab for at least the next five years. (Shaffer et al. 1946, 658; Report MS 1940)

Others would follow these pioneering efforts. In the late 1930s Howard Aiken (1900-1973) a graduate student in physics at Harvard visited the Watson Computing Bureau and discussed his plans for an automatic calculator with Eckert and his colleagues. IBM decided to fund Aiken’s calculator, popularly known as the Harvard Mark I, officially designated the IBM Automatic Sequence Control Calculator (ASCC), which would be completed in 1944. However, IBM and Aiken had a falling out over who should properly be credited with inventing the machine, among other things, and Aiken would never again work with IBM. (Goldstine 1972, 111-112)
Also during the Second World War, many new computing laboratories, modeled after Eckert’s lab at Columbia, sprang up. For example, the initial use of punched cards at Los Alamos, key site of the American nuclear bomb, had been suggested by Dana Mitchell (1899-1966), a physicist from Columbia working there. Mitchell remembered the successful punched card applications in astronomy at Columbia. (Debus 1968, 1189; Metropolis and Nelson 1982, 350) During the war, dozens of punched card laboratories would be set up by scientific and engineering groups and companies using many of the techniques pioneered at the bureau at Columbia. For example, the U.S. Naval Proving Ground at Dahlgren, Virginia, obtained IBM machines during the war, after its director, Clinton C. Bramble, met with Eckert. (Bramble 1950, 99) Another illustrative example of Eckert’s legacy is that the Aberdeen group in charge of ballistics calculations attempted to hire Lillian Feinstein away from the Columbia Bureau in 1942. (Schilt MS 1942) Again and again Eckert’s punched card techniques found important applications in diverse fields.

Eckert even provided advice to Boris Numerov on setting up a punched card shop, around 1936. Numerov never created a machine laboratory of his own. Eckert became concerned when he ceased to receive communication from Numerov and concluded that Numerov was dead by 1940, when Numerov was in fact still alive. Eckert worried that Numerov’s interest in Eckert’s installation might have led to accusations of treason by the Soviet government and precipitated his death. In 1940 Eckert received communication from another Soviet citizen, working for the Amtorg trading corporation, interested in the Watson Bureau’s equipment. However, there is no record of them actually succeeding in touring the facility. (Eckert MS 1940c; Grier 2005, 194, 248-249)

An important innovation of the punched card era that Eckert embraced was the ability to easily copy information in punched card form. In 1945 Eckert wrote an article on the tables of functions available on punched card for distribution. In fact he announced that the Watson laboratory would keep a record of the location and nature of such tables. In his 1940 book, Eckert had announced a smaller list of punched card tables available from
the computing bureau. Perhaps his most successful effort in this regard was his punching of the Boss star catalogue onto cards, since he would claim in 1967 that this catalogue was still in use. Yet, by 1946, Eckert’s enthusiasm for collecting tables on punched cards was moderated by the realization that different projects, with different requirements of precision, often required different tables in punched card form. (Eckert 1945, 433; Eckert 1940a, 115; Eckert 1940b, 376; Eckert MS 1967a, Part I, p. 21; Eckert 1947a, 80) This was an example of the utility of punched cards, with their ease of copying and their capacity to be shared with other researchers, they reduced the duplication of effort. It also suggested the importance of the ease of copying and sorting machine readable digital information that has since become a hallmark of computer technology.

Eckert’s success was all the more remarkable for the materials with which he worked. The speed of the punched card machines was not all that impressive. As Eckert himself pointed out, desktop machines could be equally fast if the calculations were not repeated. (Eckert 1940a, 25) Indeed, a race between well organized human computers armed with desktop calculators and the IBM accounting machines at the Los Alamos lab, during the war, showed that the advantage of the IBM machines was they did not get tired, not raw speed. (Feynman 1980, 125) Also, while the the accuracy of the machines was an advantage compared to the error of hand computation, this accuracy was dependent in part on constant checking by the operator. Eckert made a point of emphasizing the need for careful checks of all the calculations (Eckert 1940a, 25-29). Another example of how accuracy needed to be worked for was the preference of Eckert and Comrie for the method of differences with its ease of checking. Finally, the range of operation of the machines were often quite limited. Yet Eckert found ways to bend them to even the most complex sets of calculations. Success in using punched card machines depended as much on skilled and prudent use as the power of the machines.

The two techniques that Eckert made the most extensive and innovative use of with the punched card machines were Cowell’s method and Airy’s method. Both of these
methods promised to achieve greater mathematical precision. The origin and history of these methods were noted in Chapter 2 of this thesis. Eckert adapted these techniques to punched card machines and extended their range of applications. These techniques continued to be important later in Eckert’s career and will return in later chapters of this thesis, looking at their application on computers.

Perhaps the most important aspect of Eckert’s pre-1945 work was the relationships he built. Starting in the computational by sophisticated world of celestial mechanics, he built relationships with other astronomers and, more importantly with the technical people and management at IBM. He in turn used these relationships to promote punched card computation more widely. This made him the natural choice to lead the NAO during its modernization efforts. It would also prepare him for the next stage of his career.
Chapter 4

Ever Onward - Eckert at IBM

As the end of World War II approached Eckert continued his work at the Almanac Office. Correspondence from 1940, when he took up the position, suggests he understood that it would be his position for the rest of his career. Originally he was offered the position with a promise that he could continue and expand his scientific researches and that he “would go down in history as a second Newcomb.” (Hellweg MS 1939) Eckert himself suggested in his acceptance of the letter that the prospect of scientific work was key to his decision to go to the NAO. (Eckert MS 1939a)

However, the war and the pressures of modernizing the work of the office apparently left little time for scientific research by Eckert. The only research paper authored by Eckert during the war years was a note on a problem in calculating occultation data for the Moon in the *American Almanac*. Occultation occurs when one body moves in front of (occults) another and is useful in astronomy because it allows precise measurement of the occulting body. The position of a star occulted by the Moon would be determined (reduced) from its mean position by adding certain quantities due to the motion of the star and others due to things such as the stellar aberration. The NAO’s publication had not calculated these two in an equivalent way. (Eckert 1942, 95) Despite the important practical nature of Eckert’s work as discussed in the previous chapter the work fell short
In terms of scientific research.

In light of this, his acceptance of an offer from IBM to head up a new “Department of Pure Research” in March of 1945 is understandable. The offer had been made in 1944 by the president of IBM, Thomas J. Watson Sr., himself. As described in the previous chapter the efforts of Eckert and others and the pressures of the war had lead to an expansion of scientific efforts involving IBM machines. Also, Thomas J. Watson Sr. had always emphasized the importance of education and science as principles of progress within IBM and for the world in general. The purpose of the Pure Science Department was to bring scientific expertise into IBM and keep the company abreast of developments. At the same time, it was stated from the outset that the Department was to carry out research and education in cooperation with the academic and scientific community. (Brennan 1971, 11-13)

Eckert became the first research scientist, with a PhD, to be employed by IBM. Before this IBM employed no researchers with advanced degrees (Masters or Doctorates). Unlike companies such as AT&T and General Electric (GE), which had laboratories staffed by research scientists, all the technology and innovations at IBM were developed by teams of skilled tradesmen and a few men with bachelors degrees in engineering. Many of the inventors who worked for IBM had worked their way up through the company from the bottom. (Bashe et al. 1986, 523-527; Akera 2007, 225)

4.1 Setting up the T. J. Watson Scientific Computing Laboratory

Eckert’s first duty as director of the Pure Science Department was to form and manage the Thomas J. Watson Scientific Computing Laboratory.¹ Eckert argued that a university setting would be the ideal place, allowing easy consultation of the academic library and

¹Not to be confused with the Bureau, still in existence as a distinct organization at this point in time.
people necessary for research. Columbia University was chosen as the site of the new Laboratory; presumably because of Eckert and IBM’s close association with Columbia, IBM Headquarters was in Manhattan and Watson Sr. had been a trustee of the university since 1933. (Bashe et al. 1986, 524) Eckert’s choice of location also seems to reflect his preferred lifestyle. On leaving Columbia in 1940 he commented to the Columbia University President: “The decision to leave Columbia was not an easy one to make, for the academic and personal liberty which I have enjoyed here for thirteen years has meant much to me. I realize only too well how few places there are where all questions can be discussed on their merits.” (Eckert MS 1939b) Eckert clearly enjoyed the privilege of academic freedom. In 1946 he returned to the faculty of the Columbia University Astronomy Department, although his salary was paid by IBM.

Eckert’s preference for the University also reflected his view of how best to conduct science. In corresponding with a colleague about arrangements for a proposed award to young astronomy students he made the following comment:

> Personally I am not a great enthusiast about prizes, etc., though I am not opposed to them. I think it is undesirable to continually add to the “machinery of science” - committees, reports, awards, etc., all of which tend to dilute the efforts available for the thing of primary importance. The two fundamentals in promoting astronomy and stimulating interest in young people seem to me to be the live editing and criticism of papers in the journals and attendance and critical discussion at the meetings. (Eckert MS 1949)

The way Eckert ran the Watson lab reflected Eckert’s disenchantment with the “machinery of science” and preference for liberty. Under his directorship laboratory workers were given something of a free hand. His graduate student Harry F. Smith confirms this. He notes that as he continued at IBM demands for justification and progress reports became more stringent. (Smith MS 2007)

Soon after the lab’s inception IBM’s second PhD. Herbert Grosch was hired. Grosch had been doing work in the optics industry and upon learning of Eckert’s new role from a newspaper article wrote to ask if he could do optics calculations at the Laboratory in
the off-hours. This led to Grosch being pressed into service at the new IBM lab by a
government agent. Grosch’s first job of the new organization was of grim significance. The
Los Alamos laboratory responsible for building the atomic bomb needed more computing
power to finish their calculations relating to the atomic bomb. The Astronomical Bureau
was tied up with other war work as were other computing facilities and so it fell to the
new Watson Laboratory to organize the calculations. They were supplied with a set of
standard punch card machines for the task. Grosch and Eckert had some trouble with
the calculations because they had little experience in the solution of partial differential
equations, but the operation was a success. With the end of the war in August of 1945
the Laboratory finished this work and began normal operations. (Grosch 1991, 27-31;
Eckert MS no date B)

Eckert hired several other significant people at the laboratory, including the accom-
plished theoretical physicist Llewellyn H. Thomas (1903-1992) and mathematician Robert
“Rex” Seeber (c.1910-c.1970). Thomas had made significant contributions to describing
the behaviour of the electron, including calculating the Thomas Precession of electron
spins. (Brennan 1971, 15) Seeber was a former actuary with a bachelor’s degree in math-
ematics from Harvard. He worked with calculating machines, including IBM machines,
first at John Hancock Mutual Life Insurance and then during the war in operations re-
search. In 1944 he became an assistant of Howard Aiken’s working on the Harvard Mark
I (aka IBM ASCC). Seeber had left Harvard at his first opportunity in part because of
a disagreement with Aiken over design principles for the Mark I’s successor machines.
Seeber had suggested that both instructions and the numbers used in the calculations
(data) be stored in the same format. Aiken did not see the value in this approach and
the three subsequent machines he designed maintained separate format and storage for
instructions and data. (Cohen 1999, 162-163, 270-272; Bashe et al. 1986, 48)

Eckert also hired people with an eye towards the future of computing. Specifically in
1945 he hired Byron Havens, Robert M. Walker and John J. Lentz from the Radiation
Laboratory at MIT (involved with the development of radar) on the advice of I. I. Rabi (1898-1988). The hope was their expertise in war time electronics could be used in the development of new calculating machines. However, such developments were slow in coming and these men also did work in more fundamental research. (Brennan 1971, 16; Bashe et al. 1986, 679)

In furtherance of its purpose as a computing laboratory, IBM provided the Watson lab with a full line of its commercial calculating machines, including six multipliers and also some special machines. Chief among the special machines were two high speed relay calculators, known as IBM pluggable sequence relay calculators or Aberdeen relay calculators. These machines had been developed for ballistics calculations done at the Aberdeen naval proving grounds during the war. Numbers were read in and results fed out through punched cards but the calculators were capable of doing operations consisting of up to 96 steps on the numbers from a single card. The sequence of operations was controlled via plugboard. The relay in their names refers to the electromagnetic relays that were their main component. An electromagnetic relay uses an electromagnet to switch (open or close) a circuit and thus manipulate electric flow to other components, including other relays. Earlier IBM machines had included a few relays as control devices, but the Aberdeen calculators did all arithmetic using relays including storing numbers as sets of relays opened and closed in some combination. The Aberdeen relay calculators were the fastest relay machines then in operation at about one 6 digit by 6 digit multiplication in 0.15 seconds. (Eckert 1948a) Since relays included moving parts their speed was ultimately limited compared with vacuum tubes and other electronic devices that could also be used for switching.

There were also various completely experimental machines at the laboratory, including two relay calculators designed by IBM inventor Pete Luhn (1896-1964) and a specialized analog device for solving linear equations using a method suggested by Columbia Professor Francis J. Murray (1911-1996). One of the relay calculators was designed to
minimize the use of relays and consisted of a modified keypunch and a series of readers and punches that could carry out a number of calculations and was controlled by a set of master cards. The other relay calculator performed only multiplication or division and worked by feeding signals to and from an IBM accounting machine. This addition would allow an accounting machine to perform complex sequences of calculations. (Grosch 1991, 84-85; Eckert 1947a, 76-80) It is not clear how much actual use these machines saw, but their presence illustrates the way the lab was provisioned in its early days.

4.2 Development and use of the SSEC

One of Eckert’s main responsibilities, in the early years of the Watson Lab, was the design of the new IBM large scale electronic calculator that would become the Selective Sequence Electronic Calculator (SSEC). (Brennan 1971, 15-16) At first this machine was known as the Super Calculator, but soon became known as the Sequence Calculator. Plans for the new machine seem to have originated soon after the dedication of the Harvard Mark I. Eckert understood from the time of his initial hiring that part of his mandate was to help in the design of a new large-scale electronic machine. One clear objective was to surpass the earlier Harvard machine that IBM had built. (Bashe et al. 1986, 47; Eckert 1967a, Part II, 21-23) Eckert’s own use of the SSEC will be covered in the next chapter. However this seems an appropriate place to discuss the construction and use of the machine.

First some background on developments in computing. 1946 saw the unveiling the first large scale electronic calculating machine the ENIAC (Electronic Numerical Integrator and Computer) at the Moore School of Engineering. Developed under a contract with the US army for Ballistic calculations, the ENIAC used electronic circuits, such as vacuum tubes, to do long series of calculations. It lacked the flexibility in its operations found in automatic relay machines like the Harvard Mark I or the Bell Mark V calculator.
(also unveiled in 1946) that ran a program from paper tape. At first the ENIAC’s wiring had to be physically rearranged to prepare it for different problems. However, the ENIAC was unrivaled in terms of raw speed as it was capable of performing a multiplication in 2 milliseconds whereas the Bell relay calculator took about a second to do a multiplication. (Goldstine 1993, 115-118, 137) The ENIAC therefore heralded a new era of large electronic calculating machines.

There was a slight connection between the creators of the ENIAC and Wallace J. Eckert’s work. John Mauchly, one of the masterminds behind the ENIAC, was a physicist who worked on meteorology in the 1930s and early 40s. In the course of learning about statistics and carrying out statistical calculations in meteorology he became aware of the uses of punched cards by mathematicians and scientists. Apparently one of his sources of knowledge of these techniques was Eckert’s 1940 *Punched Card Methods*. However, the depth of his knowledge of Eckert’s book is unclear. In a 1970 interview Mauchly recalled Eckert as being concerned with using progressive digiting, a method of doing multiplications by repeated additions. (Mauchly MS 1970, 14) The progressive digiting technique was widely used in the 30s and 40s for punched card methods, and Eckert does mention this technique in his book. However, as detailed earlier, much of Eckert’s work focused on doing multiplication directly with the IBM multiplier.

The design of the ENIAC had also led to several conceptual developments by its designers, Presper Eckert (1919-1995) and Mauchly, along with Herman Goldstein, John von Neumann and various other engineers. Specifically, in 1944 and 45 this team conceived and laid out features for the successor machine, the Electronic Discrete Variable Computer (EDVAC), including separating the functions of the machine into various components of the new machine in terms of control, arithmetic, high-speed memory, slower internal storage and input and output units. The design process also generated other key ideas: storing instructions and data in the same format, serial processing of instructions and use of a simple addressing format for instructions. These concepts, as described in
von Neumann’s “Draft Report on the EDVAC”, would serve as a model for many of the first computers developed in the late 40s and early 50s. Also, the features described in this report became known as the “von Neumann architecture” and became the model for most, if not all computers in the subsequent decades. (Ceruzzi 1998, 20-24) This major developments in design served to answer the problem of how to provide continuous instruction and control to a machine that operated too quickly for direct human intervention.

In addition to Eckert, the two other key people involved in the design of IBM’s Sequence calculator were Francis “Frank” E. Hamilton (1898-1972) and the aforementioned Seeber. Hamilton, an experienced IBM inventor, began his career in 1923 as a draftsman. Hamilton had supervised much of the production of the Harvard Mark I and so was a natural choice to head up the production of the new calculator. Discussions began in informal meetings and correspondence between Hamilton, Eckert and J. C. McPherson, IBM’s director of engineering. Around March of 1945 plans for the machine remained embryonic. For example, after prompting by McPherson Hamilton suggested a simple relay machine possibly without any extensive ability to carry out complex sequences of instructions. (Hamilton MS 1945a; Bashe et al. 1986, 26)

Eckert’s duties organizing the new laboratory at Columbia limited his ability to contribute to these discussions. Seeber became his liaison with the IBM engineers designing the machine and contributed to the specifications of the new machine. As a result the SSEC implemented Seeber’s idea of storing instructions and data in the same format. This allowed the machine to perform different instructions depending on the outcome of intermediate calculations. This concept is sometimes called the stored-program concept and is often considered the defining characteristics of the von Neumann architecture. However, Seeber apparently arrived at this idea independently of von Neumann’s ideas. On September 26 and 27, 1945, a meeting was organized at the IBM factory in Poughkeepsie, New York, and at this time plans for the machine became serious and more focused.
By the end of October a tentative list of features for the machine had been put forward, including using electronics for arithmetic calculation. (Bashe et al. 1986, 46-50; Cambell-Kelly and Aspray 2004, 81; SSEC MS no date; Hamilton MS 1945b; Hamilton MS et al. 1945; McPherson MS 1947)

Machines like the SSEC that automatically carried out a sequence of instructions were relatively new at this time. Eckert referred to such machines as sequence calculators. The sequence calculators Eckert was familiar with lacked the speed of electronics and the full versatility of control of later machines and so are not completely of a kind with modern computers. In 1946 Eckert felt the need to sound a note of caution about the enthusiasm that had emerged for the new machines. Eckert pointed out that a large sequence calculator would only use a small portion of its resources at a given time. In comparison an IBM tabulator could perform multiple additions simultaneously on the same cycle of a card through it. Eckert suggested that when it was possible to avoid sequence calculation simpler non-sequence machines able to use all their facilities at once would prove more economical. As it turned out creating fast machines capable of parallel (simultaneous) operation would prove a major problem for later designers and so sequence machines in the form of modern computers came to dominate. (Eckert 1947a; 77-78; Ceruzzi 1998, 23-24)

In a slightly later writing Eckert describes a similar dilemma in terms of the decision of whether to calculate a function, \( f(x) \), sequentially or in parallel. The function is imagined to be the sum of various terms with different terms requiring different arithmetic operations. Eckert suggests that it is often more efficient to set-up to calculate individual terms for all the \( x \) values required separately and then add the results to get the various values of \( f(x) \). Eckert’s conviction on this issue clearly relates to work done by hand methods and using traditional punched card equipment. (Eckert MS no date C, Chapter IV pp. 2-3) It does not seem that Eckert’s conservatism about machine design dulled his enthusiasm for the SSEC, but the question of parallel versus sequential operation is
Although Seeber would more often attend the meetings, Eckert was still responsible for producing the specifications of the machine based on these discussions. In December 1945 Eckert produced a set of specifications of the mathematical functions the machine would perform with some indication of the necessary machine elements. For example, he specified the size of relay and tape storage, that multiplication and division should be electronic and that a multiplication should take 20 milliseconds. However, he often left what components or the number of components open to further revision. For instance, Eckert left it undecided what form the machine’s high speed table look-up units would take.\footnote{The idea of high speed table look-up was to have a specialized means of searching and retrieving data stored in tabular form, i.e. as some systematic series of the values of a function. This would allow the tabular data to be used more efficiently than the other data stored by the machine, presumably in a non-systematic series.} This despite the fact that, according to Herb Grosch, Eckert had been insistent on the machine having table look-up capabilities. Eckert also advised that, since the machine was scheduled to be completed in one year, they should make do with the technology available and not attempt to develop new technologies. Hamilton found Eckert’s specifications inadequate in their technical detail and, working with Seeber, Hamilton responded with a set of technical specifications, which Eckert approved in March of 1946. These specifications gave details of all the features the machine would possess in an almost final form, including specifying the form instructions would take and how the machine would specify the next instruction. Throughout 1946 Seeber, Hamilton and a team of IBM engineers worked at the IBM factory in Endicott, New York, on constructing the machine. The machine was fully assembled and tested by the end of 1947. (Eckert MS 1945; Hamilton MS 1945b; Hamilton MS 1946; Grosch 1991, 8)

The SSEC was a large machine (see figure 4.1) consisting of 12,500 vacuum tubes, 21,400 relays, 66 paper tape readers, including 36 designed for quick look up of tabular values, two standard punched card readers, a card punch and two printers. Three massive
Composite view of the SSEC with floor plan. (Eckert 1948b, viii)  

Figure 4.1
rolls of paper tape were housed in the machine each with a punching unit to store numbers and usually 10 readers, but all 30 readers could be placed on the same tape as appropriate to the problem. Tables of values could be stored and read off much shorter loops of punched tape quickly by the 36 readers intended for tabular values. Further efficiencies in table look-up were achieved by having a special mechanism that searched through these loops independently and more quickly than the machine’s normal tape handling procedures. This ensemble of short loops of tape with a special searching mechanism was the high speed table look-up. The machine could output results via a printer or punching to standard IBM cards. After being put through its paces at IBM’s Endicott laboratory in 1947, it occupied a large room in a building on Fifty-Seventh street next to IBM headquarters at 590 Madison Avenue, New York, until it was dismantled, in July of 1952, to make way for the first IBM 701. (Eckert 1948b, 319-320; Bashe et al. 1986, 54-57)

Numbers were represented in the machine in binary-coded decimal, that is 4 two state components (such as vacuum tubes), each component representing a bit (binary digit), were used to represent a decimal digit from 0-9. This contrasts with the pure binary system used in many electronic computers that were soon to be produced (and is now near universal in computing). Pure binary is a more efficient storage mechanism using all the possible states of the components, 4 bits capable of storing 16 distinct values rather than 10.\textsuperscript{3} Also, the arithmetic mechanisms for decimal arithmetic are usually more complex, but also faster, than those of binary arithmetic. The electronic arithmetic circuits of the SSEC were based off those used in the prototype for the IBM 603 electronic multiplying punch, a far simpler machine that operated more in-line with IBM’s other punched card devices. The punch-tape used by the SSEC was actually standard IBM card stock of 80 column width, but the space for the first and last column were used for

\textsuperscript{3}Some examples of binary coded decimals would be zero as 0000, five as 0101, nine as 1001 and eleven as 0001 0001 with values such as 1011 not represent a number, but might be used in some way. In pure binary representation would render zero as 0, five as 1 0 1, eleven as 1 0 1 1.
the sprocket system that moved the tape. Rather than being coded directly in decimal, one column and ten rows to specify a digit, as in ordinary applications, the digits were punched in binary coded decimal (the presence or absence of a hole being a bit) read across 4 columns in one row for each digit. (Eckert 1948b, 319, 321; Bashe et al. 1986, 51)

Numbers were represented by an entire line of such paper tape (78 bits) as 19 digits plus sign (2 bits or half-a-digit) in a fixed point system so that the magnitude was indicated by the digits as well, as opposed to floating point or scientific notation where the digits and magnitude are separated. 8 such 19-digit numbers could be stored in vacuum tubes, 150 in the slower relay system, 20 000 on the large paper tape system and 5 000 on the table look-up. The machine could perform up to 14 by 14 digit multiplication in 20 ms (milliseconds) giving a 28 digit result, a division of such numbers in less than 34 ms, and addition or subtraction of 19-digit numbers in less than 0.3 ms. The various components operated at different speeds, numbers could be retrieved from electronic storage in less than 1 ms, retrieval of numbers in relay storage took 20 ms, accessing a paper tape reader also took 20 ms, punching took 40 ms, and the paper tape could move at a rate of 1 line in 20 ms. In the case of the table look-up, this speed combined with the 36 readers distributed over as much as 5000 lines of tabular data led to an estimated average look-up time of 1 second. Transfers of data from storage could occur simultaneously along 8 different tracks. Input and output occur at a slightly slower rate, each of the two punched card readers operated at 200 cards a minute or about 80 ms to read a single 20 digit number, the card punch also operated at 200 cards a minute, and the two printers could print about 30 000 digits in a minute or 80 ms for one printer to print a single 19-digit number. (Bashe et al. 1986, 50, 55; Eckert 1948b, 315, 319-320)

The SSEC’s instructions or lines of sequence were made up of two of these nineteen digit numbers, but each number or half-line usually acted as a separate parallel instruction. Each instruction included three portions indicating the location in storage
of numbers to be acted on (called P, Q and R in the first half-line and T, U, V when found in the second line), a portion to indicate the arithmetic or other operation to be performed (abbreviated OP) and a portion indicating the location of the next half-line of instruction (designated Seq). A typical half-line would be to take a number from one location, multiply it by a number from another location and place the resulting product in a third location and specify the next half-line of sequence. Apparently though, operations could be undertaken across both half-lines such as adding five digits and putting the sum in a sixth location. Since each half-line of sequence or instruction refers to four storage locations or addresses (three number locations and the location of the next instruction) the SSEC has been called a four-address machine. (Eckert 1948b, 319; Bashe et al. 1986, 585-587)

A later description of the SSEC, by Eckert, explained that it could split the storage of numbers into two, storing two 9 digit numbers, instead of one 19 digit number, in a given location. This was presumably a later modification since the SSEC was subject to modification over the course of its existence. The large size of the numbers and instructions in the SSEC was a hallmark of early machines and later machines would have much smaller standard number or “word” sizes. The Harvard Mark I used 23 digit numbers and operators often found it expedient to disable some of these digits to speed computation. The large number of digits also increased greatly the number of components of the machine. The main reason for the relatively large number of digits was the absence of floating point. A large number of digits were useful for fixed point operations in order to help ensure that the significant digits were carried through, along with the place holding zeroes, in operations like multiplication.  

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4Imagine a problem that involves multiplying many integers that vary in size from 4 to 8 digits. The products then vary in size from 16 to 7 digits. It would be complicated to keep the significant figures on a computer that used an 8-digit word size. Imagining the intermediate product to appear in a 16 digit temporary store, the programmer would have decide which 8 digits to preserve. If he kept the first eight this would achieve maximum preservation of precision for the 16 digit product, but would lose all information about 7 and 8-digit products. In order to achieve a preservation of precision some complication to the program would be required, for example one could break up the integers into groups.
of the SSEC to operate with 9 digit numbers and the success of the ENIAC with 10 digits suggests that their may have been a failure to appreciate what the actual needs and tradeoffs would be during the design stage. (Eckert et al. 1954, 347; Cohen 1999, 169-176; Brooke and Backus MS 1951, 1, 4; Brooke MS 1981, 3, 4)

The architecture of the SSEC provides the best evidence of the independent origins of the SSEC’s designs from other developments. Since the key design ideas for the EDVAC had been formalized by June of 1945, with the release of von Neumann’s draft report, there may, in theory, have been sufficient time for Eckert, Seeber and Hamilton to become aware of the ideas and incorporate them into the machine. Also, Eckert, McPherson and others at IBM had contact with von Neumann, through the work IBM was doing for the Manhattan Project. Indeed, in a letter of March 1945 McPherson mentions being inspired by von Neumann’s thoughts on division by machine. However, most machines built in the mold of von Neumann’s draft report would include only a single address in each of their instructions and so a one-address architecture was far more common in early computers, such as the Cambridge EDSAC, Institute for Advanced Study machine and UNIVAC I. The von Neumann architecture also recommended serial operation (doing one thing after another) whereas the SSEC’s architecture engaged in some parallel operation. The distinctive architecture of the SSEC corroborates the claims of its designers that they developed its design independently of the ideas surrounding the EDVAC design. (Grosch 1991, 29-31; McPherson MS 1945; Ceruzzi 1998, 20-24; Seeber MS 1967)

As the dedication of the SSEC in January 1948 approached, Eckert apparently became worried about what use the machine would be put to. He wrote a letter to various IBM executives warning against hiring the machine out to the highest bidder. He also noted

by digit size before multiplication and have different routines for each group.

Due to significant changes in design after von Neumann’s report, the EDVAC as constructed was a four-address machine and did every calculation twice in parallel as a check on errors. However, the EDVAC’s architecture still differed significantly from the SSEC for example it was a binary machine. Also, the EDVAC’s design was not finalized until May of 1947 by which point the SSEC was designed and almost completely built. Therefore, the resemblance is most likely coincidence. (Williams 1993, 26-29; Godfrey and Hendry 1993, 13)
that the highest bidder would inevitably be the government or rather the military. Eckert pointed out that the SSEC would be in a unique position to aid scientists interested in the pursuit of knowledge for its own sake, since all the other large calculating machines currently in operation were under exclusive government contract for military work and so not available for use in broader scientific work. He argued that the key difference between commercial and scientific work was not the content of the work, but whether the emphasis and goal was the advancement of knowledge or the saleability of the result. He also argued that scientific instruments were used to greatest effect when they were entrusted to the hands of high caliber scientists. “No scientific laboratory has been greater than the caliber of the scientists in it; great discoveries have not been made by remote control through committees, regulations and divided responsibility.” (Eckert MS 1947) Eckert admitted that one could not count on any financial reward from pure scientific research, but he pointed out that his own rent free punched card laboratory had payed excellent dividends to IBM in rent paying laboratories that had been modeled after it. He also admitted that some work vital to national security would probably need to be done on the calculator, but urged that IBM make the new machine a high priority in its endowment of pure science. (Eckert MS 1947)

On January 18th, 1948 IBM President Thomas J. Watson dedicated the machine to the use of science throughout the world. During the dedication the problem run was one of Eckert’s choosing, computing positions of the Moon directly from Brown’s theory; more on this in the next chapter. (Brennan 1971, 22) Subsequent jobs on the SSEC would often involve work done on a contract basis, although compensation was always on the basis of cost and not at a profit. Overall use of the machine was controlled by the Department of Pure Science and later the Department of Applied Science. Problems came either directly from Watson lab scientists, scientists with sponsor in the lab or through IBM’s existing customer relationships. (Bashe et al. 1986, 57; Grosch 1991, 81)

Operation of the machine was complex and required a large team of skilled operators.
Seeber was overall supervisor of the machine. Eight IBM engineers were responsible for maintaining the machine, which was a significant undertaking. An additional four engineers were responsible for the massive air conditioning system required to deal with the heat generated by the thousands of vacuum tubes. Also, required were programmers, who prepared the problems in machine code with two or three working as programmers for a problem, more for larger projects. In total over two dozen men and women would work as programmers for the SSEC. In addition, often, one of the Watson labs scientists such as L. H. Thomas consulted with the clients to render the problem into a form that was tractable to the SSEC’s modes of operation. (Brooke MS no date, A1, A2; Brennan 1971, 23-26)

The operation of the machine was not always smooth. During the process of building the machine errors would occur and the sources of these errors would then need to be found and corrected. Some errors were intermittent and hard to track down and as a result were never completely eliminated. Famously during the day of the dedication an unidentified problem caused the machine to stop several times during practice calculations, however when the time came for the dedication it worked continuously throughout the ceremony. Also, even at the time of the dedication certain systemic deficiencies remained. The electronic division mechanism was not in working order, but this was not a problem because the dedication problem involved no division. Finally the tape punches required significant redesign as problems were discovered with their operation and they never worked as specified. The verification mechanism for the punches could not operate at the high speed the machine was to be operated at and so had to be removed. (Brennan 1971, 23; Brooke MS no date, 39-42; Hamilton MS 1945b)

I have found no published summary of the projects that the SSEC undertook. However, A. Wayne Brooke (1913-1996), the supervising engineer of the SSEC during its use, had discovered, during his own research on the history of the SSEC in the 1980s, a copy of an anonymous undated summary of work that he found credible. On the basis
of the style of writing and its content Brooke attributed it to H. Kenneth Clark, a programmer for the SSEC. Much of what follows is based on this source, with references to corroborating sources where they are known. I have discovered one gap in the summary, the work on the fluid-drop model of atomic fusion by D. L. Hill (1919-). In total about 24 projects ran calculations on the SSEC. Another eight projects were proposed for the SSEC but never run due to cost or time constraints. Of the projects run on the SSEC, three were proposed by Eckert: the dedication problem, the numerical integration of the outer planets and a further set of calculations from Brown’s theory. (Appendix B MS no date, pp. B1, B8, B12; Hill 1950, 9) All of Eckert’s work will be detailed in the next chapter.

4.3 Projects for the SSEC

Others from the Watson lab would also use the SSEC for calculations. L. H. Thomas proposed the second problem for the machine, calculation of the statistical electron distribution and electrostatic potential (also called statistical fields) for various atoms and ions. Although begun in 1948, calculations continued to be made into 1950. Also, John Sheldon, a PhD student working under Thomas, did two sets of his own calculations on the SSEC for his research on the statistical fields of molecules, in 1951 and 1952. The third problem run on the SSEC was an internal IBM accounting problem relating to the company’s pension plan. The fourth problem involved calculations of shock wave refraction for R. J. Seeger and H. Polachek (1913-2002), researchers at the Naval Ordnance Laboratory, White Oaks Maryland. This project was the first one done on a pay basis and took place about six months after the dedication of the SSEC. This problem took a month to solve, most of which was spent preparing the problem, the actual machine time used was 60 hours. (Appendix B MS no date, B1-B4, B11; Thomas 1954, 1766; Sheldon 1955, 1291, 1299, 1301; Polachek 1950, 122) Shock waves, fluid mechanics and air flow
problems provided several problems for the SSEC.

Due to the long preparation times multiple projects were ongoing at a given time. Some could remain in progress for a long time, as in the case of a problem entitled “On Compressible Flow in the Sub-sonic Region.” Hans Kraft of GE proposed this as part of a jet turbine design project. The proposal to use the SSEC was made in the spring of 1948, but machine computation did not begin until June of 1952, the final month of the machine’s operation. (Appendix B MS no date, B4; Kraft 1950, 66)

One of the most prominent problems in fluid mechanics dealt with was the stability of Plane Poiseuille Flow. A controversy had arisen between mathematicians C. C. Lin (1916-) and C. L. Pekeris (1908-1993) as to whether such flow became unstable. Two separate attempts were made to answer the question using numerical integration on the SSEC, one by John von Neumann and the two disputants and the other by L. H. Thomas after the first attempt failed to settle the question. Thomas’s calculations vindicated Lin’s conjecture, based on other evidence, that the flow became unstable in a certain range. Both attempts used over a hundred hours of time on the SSEC. (Appendix B MS no date, B4, B5, B12; Thomas 1952, 813; Thomas 1953, 782)

The SSEC performed a related calculation in fluid mechanics, this time for Martin Lessen (c.1921-1999) a graduate student at MIT. Thomas sponsored the problem and it was done on an unpaid basis. The problem concerned the flow in the boundary layer between parallel streams of an incompressible fluid, which was shown to be unstable. (Appendix B MS no date, B5; Lessen 1950, 571) Two other problems in fluid mechanics were abandoned in the planning stages, both proposed by researchers of the National Advisory Committee for Aeronautics (NACA) Cleveland, the first by a Dr. Wu and the second by an R. L. Turner. (Appendix B MS no date, B9, B11)

Another prominent problem in hydrodynamics was the classified research called “Project Hippo”, commissioned by the Los Alamos Laboratory of the Atomic Energy Commission. Hippo involved the computation of hydrodynamic shock waves. Several celebrated fig-
ures were involved in the computation including John von Neumann and Adele Goldstine (1920-1964). Goldstine was an important researcher involved with the ENIAC and wife of Herman Goldstine. Hippo would prove to be the largest computation undertaken by the SSEC, requiring the SSEC to work on a 24 hour schedule. (Appendix B no date, B8, B9; von Neumann and Richtmyer 1950, 232-233)

Another classified shock wave problem was done for the Naval Ordnance Laboratory in 1951. Although I have not uncovered any details of the problem’s content, the description available does include the amounts and costs charged for services rendered. The problem took 16 hours of computer time but 718 hours of personnel time. However the cost for computer time was given as $300, while the cost for an hour of personnel time was only $5. Therefore the short run of time on the SSEC cost more ($4800+) than the hundreds of personnel hours ($3590). This suggests both the complexity of the problems selected for the SSEC and the difficulty of preparing them in a machine format. (Appendix B MS B, B11)

More classified projects were done for the John Hopkins Applied Physics Laboratory acting as a contractor for the US Navy. Two calculations were done between October 1951 and May of 1952. A third project was started but abandoned during coding because of the imminent dismantling of the SSEC in June of 1952. (Appendix B MS no date, B11) A project was done for the Nuclear Energy for the Propulsion of Aircraft (NEPA) division of Fairchild Aircraft that may also have dealt with classified content. The actual calculation involved the solution of 168 simultaneous linear equations. (Appendix B MS no date, B9)

The work on the SSEC relating most directly to nuclear fission was done openly. David L. Hill, of Vanderbilt University, carried out a set of numerical integrations of the liquid drop model of fission on the SSEC. The model proposed by Niels Bohr (1885-1962) in 1935 was an approximate model and Hill along with John Wheeler (1911-2008) wanted to see whether it could explain the asymmetry of the atomic weights of the products of
fission. These calculations took 103 hours on the SSEC. The results showed that contrary to some initial expectations the model was able to account for the asymmetry in fission products. (Hill 1950, 9, 14-15)

More mundane projects also found uses for the SSEC. The David Taylor Model Basin, a research centre in the U.S. Navy, used the SSEC in analysis of the vibrations for the design of ship hulls. Two such analyses were completed on the SSEC and a third one proposed, but never implemented. Two oil companies, Carter Oil Co. and Gulf Research & Development Co., would submit similar problems on oil exploitation for the SSEC, but they refused to allow the problem to be done as a joint effort. In the work that followed Carter obtained a successful result, while Gulf Research failed in its assigned goals. The problems are also notable because Carter Oil hired John von Neumann as a consultant for the project. The Reeves Instrument company used the SSEC to run a calibration calculation for an analog computer to be used in U.S. Navy missile guidance. Finally Watson lab researcher Herbert Grosch would begin a program to do lens design calculations on the SSEC. Due to the large size of the calculation, unforeseen difficulties and Grosch’s leaving first the Watson lab and then IBM, the computation was never completed. (Appendix B MS no date, B5-B9; Aspray 1990, 105-108)

Of the final four proposals that were never implemented, two were statistical problems, one for the US Navy and one for the U. S. Census Bureau, one was a radar problem for Sperry Gyroscope and one was an actuarial problem for the Prudential Insurance Company. (Appendix B MS no date, B4, B5) The SSEC was mostly used for scientific and engineering problems involving the solution of differential equations or solving systems of linear equations, some proposed problems suggested the potential of the computer to solve problems in business.

In total, government or private business contracts accounted for 12 of the jobs that actually ran on the SSEC, while 11 of the jobs run were done at the request of scientists and with IBM footing the bill and one job was IBM’s internal accounting calculation. So
numerically at least an almost even split was achieved between monetary interests and Eckert’s ideal of providing the machine for the pursuit of knowledge. This seems to have been the position that Eckert anticipated or was negotiating for when he acknowledged the need for work of national importance to be done on the SSEC. While IBM had hired out the machine, they did not auction its use off to the highest bidder and so broke the military monopoly on large scale high speed computers in the United States. The history of the SSEC demonstrates IBM’s commitment to the goals of supporting science as Eckert espoused, but also the limits of those goals.

### 4.4 Legacies of the SSEC

Despite the SSEC’s ground breaking status as IBM’s first large scale general purpose fully programmable computing machine, its impact was limited. The new design ideas created by Presper Eckert, Mauchly and the rest of the ENIAC team and embodied in the von Neumann Draft Report on the EDVAC would render it obsolete. IBM’s first commercial general purpose large scale calculator was the IBM 701, first delivered to a customer in 1952. One such machine would replace the SSEC in its place on display at the IBM headquarters on Madison Avenue. This machine’s architecture was heavily based on the design of the Institute for Advanced Study computer, which had been built by a team lead by John von Neumann. As a result the IBM 701 took almost no design elements from the SSEC. The 701 was a pure binary machine with a single address instruction structure and stored in 35 bits (half the length of an SSEC number or word). However, the programming team for the 701 included many people who had cut their teeth coding problems for the SSEC. Perhaps most notable among these programmers was John Backus (1924-2006) who would go on to create FORTRAN, one of the earliest and most successful programming languages. (Bashe et al. 1986, 138-139, 158-162, 334-336)

The IBM 650, IBM’s smaller computer based around storage of information on a
rotating magnetic drum, introduced in 1954, owed more to the SSEC. The project to produce the machine had been managed by Hamilton, chief engineer for the SSEC. Also, the original project that spawned the 650 had been to create a successor to the SSEC for wide commercial use. The 650 was a binary-coded decimal machine and it used a two address architecture, the second address being the location of the next instruction and possessed provision for high-speed table look-up. While the two address architecture may have been a descent with modification from the SSEC’s architecture, it also served to allow optimal speed. The period of time required for one rotation of the drum was a significant amount of time given the speed at which the machine calculated. Therefore better speeds of calculation could be achieved by placing the next instruction so it would appear quickly after the current operation was completed. The 650 would prove to be a very successful machine for IBM, with hundreds in use during the 1950s. (Bashe et al. 1986, 58, 72-78; Eckert and Jones 1955, 88)

The only other full-scale computer developed at the Watson lab was the Naval Ordnance Research Calculator (NORC, see figure 4.2). This machine designed by Byron Havens and the other Watson lab electronics experts started as an internal Watson lab project and as its development proceeded it was proposed to be done as a special project for the Navy. The goal became to create the most powerful machine possible with cost no object. IBM undertook the work as a contract of cost plus a fixed fee of $1. The resulting machine was the first to operate with a basic clock speed of 1 megahertz (a million cycles a second) and it is sometimes called the first supercomputer. The machine’s hardware was far more reliable, faster and novel than the SSEC and its logical design included more mature computer features such as floating point numbers and built in error checking. Still, the machine had a few similarities to the SSEC. Specifically the numbers were coded using binary-coded decimal and the address structure was non-standard. The NORC used a three address architecture usually two numbers to be operated upon and a result, reminiscent of the SSEC; though the location of the next instruction was handled
automatically. There is at least some suggestion, from the recollection of IBM employee John Lentz, that Eckert advocated the use of the three address structure on the basis of ease of preparing instructions in that format. The use of binary-coded decimal may also have been another feature he argued for since he commented that it was easier to understand the processes of decimal machines. However, overall Eckert apparently played far less of a direct role in the development of the NORC compared to his work on the SSEC. (Lentz MS 1980, 28; Eckert and Jones 1955, 89; Bashe et al. 1986, 132-133, 181-183)

The limited impact of the SSEC is also reflected in its depiction in the history of computers. The machine rarely receives any extended descriptions. As a result accounts of it are often misleading or contain false statements. An example of one such confusion is to attribute its design to Eckert, whereas Seeber and Hamilton deserve much greater credit. Similarly some histories suggest the work Eckert did on the SSEC was the majority of what was done. Another common misattribution is to fail to appreciate how instructions and data were held in the same storage. Specifically, some describe the SSEC as not being a stored-program computer even when they define such a computer as a machine that holds data and instructions in the same storage. However this confusion can be attributed to the ambiguity of the definition. Two common conditions used to identify stored-program computers are that they should be electronic and that the whole program should be stored in memory for use. In this regard the SSEC’s heavy use of relays and the small size of its fast memory count against it. Also, the instructions for the machine were most often read directly off paper tape without modification. (Randell 1982, 379)

The SSEC was important in the career of Wallace J. Eckert. Although Eckert did not play a predominant role in either the creation or use of the SSEC, he did have an important role in both. Eckert’s influence on the construction and use of the SSEC was greater than for any other computer. It was Eckert who hired people such as Seeber and Thomas, who were critical in the design and use of the SSEC. Many of the projects done on the SSEC document Eckert’s commitment, backed by IBM, to providing top scientists
(Eckert and Jones 1955, ii, 17)

Figure 4.2
free access to the best calculating machines as a means of advancing science.

4.5 Eckert as Promoter of New Computing Techniques

In his first years at IBM, Eckert continued efforts to educate people about punched card machine methods. Eckert and the Watson lab staff organized classes teaching computation with machines methods starting in 1946. (Grosch 1991, 69-70, 100-101) In February 1947, the Journal of Chemical Education published an article by Eckert giving an overview of punched card techniques, as part of a discussion of punched card techniques in chemistry. (Eckert 1947c) In 1948, he wrote an article for the computation journal Mathematical Tables and Other Aids to Computation (MTAC) describing the function and use of the IBM pluggable sequence relay calculators. (Eckert 1948a) Also in 1948, Eckert published a description of the SSEC for Scientific Monthly. (Eckert 1948b) Clearly Eckert was attempting to communicate his discoveries and techniques to the world. Despite his involvement with the cutting edge of machine developments, Eckert would continue to emphasize the value of the small standard accounting machines for many scientific computation problems. (Eckert 1951, 17)

In 1940, 1946, and 1947, IBM organized Educational Research Forums. These meetings brought together users of punched cards in educational settings to present on methods and proceedings were published. Eckert spoke at the ’46 and ’47 meetings giving descriptions of the work and equipment of the Watson lab. Grosch also contributed a paper to the ’46 meeting, on the numerical analysis of orbital periods that he had performed at the Watson lab, but otherwise most of the papers were on statistical topics using standard punched card equipment. In 1948, the forum was transformed into a “Scientific Computation Forum.” The papers ranged from technical descriptions of methods of solving equations to general descriptions of calculation practices at various computing
laboratories around the country. The machines discussed included older IBM accounting machines and the latest experimental machines such as the SSEC. (Grosch 1950, 3; Grosch 1947; Eckert 1947a; Eckert 1947b) In this way Eckert and the Watson lab had another means of promoting machine computation and exploring its potential in various applications.

Eckert never completed what might have been his largest publication educating the scientific public about new developments in calculating machines, a second edition of his *Punched Card Methods and Other Aids to Computation*. Apparently first planned for publication in the early 1950s, he would occasionally refer to its impending publication in print and correspondence. (Eckert et al. 1951, XI) Why this book failed to materialize is unknown. One possibility is that with the proliferation of books on punched card (or rather computer) methods he did not see the need. Another possibility is that he did not have time to complete it, due to his commitments to astronomical research and the management of the Watson Lab.

The plans, for the second edition of *Punched Card Methods*, were ambitious. Eckert sketched out plans for 18 Chapters, versus the 12 chapters of the original. While Eckert repeated some pieces from the original, the majority of the text would have been rewritten or jettisoned. For example, the original edition had four chapters on applications all involving astronomy. In the new edition Eckert planned to include only a single chapter on applications in astronomy and three new chapters on applications in chemistry, engineering and physics. The section on techniques was much more involved than in the original publication and involved detailing cutting edge techniques in machine computation such as Monte Carlo simulation. (Eckert *MS* no date C, table of contents)

Eckert’s work in the 30s applying punch card machines to the compilation of star catalogues continued, to an extent, into the 1950s. With the help of Rebecca Jones, John Lentz and several other Watson Lab researchers and IBM engineers, he built a machine for automatically measuring the position of stars from photographic plates. It
then punched the resultant measurement on punched card. (Eckert 1948c, 177-178) In this way the imprecision and drudgery of human measurement was avoided.

In doing this he completed the automation of table making. In the 1930s, he automated the computation and organization of table data. At the Nautical Almanac, he developed ways to automate proofreading and printing of tables. Now, he automated data gathering. One of the articles he coauthored with Jones suggested his drive for automation. He remarked that for the future astronomical cataloger: “The catalogs of the future will require his scientific insight but for his routine labor will be substituted the technological developments of our age.” (Eckert and Jones 1954, 83; Olley 2006)

Eckert’s last major contribution to the literature on scientific computation in general was *Faster, Faster: A Simple Description of a Giant Electronic Calculator and the Problems it Solves*. It was coauthored with Rebecca Jones, a Watson Lab staff member, and with some text provided by L. H. Thomas. It differs from Eckert’s earlier work publicizing calculating machines in that it is aimed not at a scientific audience, but at the general public. It describes in some detail the technical features of the NORC and the machine’s logical and arithmetic operation in order to demonstrate the general characteristics of new large-scale computing devices. Although it avoids technical language it still gives a detailed picture of the elements that make up the NORC. (Eckert and Jones 1955)

In the late 1950s and throughout the 1960s Eckert continued to make some modest contributions to scientific machine computation. He sat on the International Astronomical Union (IAU) “Committee on Astronomical Records in Machine-Readable Form” in the 1960s as part of his work on the IAU’s Commission 7, the commission for celestial mechanics. (IAU 1964, 111) Similarly he chaired a 1967 IAU conference on “the Use of Electronic Computers for Analytic Developments in Celestial Mechanics.” (Eckert 1967b, 195) While there was a clear shift in Eckert’s work in later years, advancing computer and machine computation always remained a part of his work.

The Watson laboratory itself, at least in its first few years of operation, became a cen-
tre for promoting machine calculation. As mentioned with the two pluggable sequence relay calculators at its disposal (from late in 1946), it had one of the best equipped computing machines facilities in the world. Scientists would visit the laboratory to have calculations done or to learn how to perform the calculations on machines themselves. To some extent this continued the work of the Astronomical Computing Bureau. For example physicist Martin Schwarzschild (1912-1997) with the aid of Lillian Hausman (formerly Feinstein) used first the Bureau’s IBM 601 multiplier and then the Watson Lab’s pluggable sequence relay calculator to develop techniques to solve differential equations. (Feinstein and Schwarzschild 1941; Hausman and Schwarzschild 1947) However, the scale of the operation had increased. One of the more notable computational tasks undertaken at this time was the calculation of the Kleine Planeten, a tables of the orbits of the minor planets (asteroids) whose continuation had been put in jeopardy by the damage to Germany in the war. Herb Grosch oversaw these calculations. (Grosch 1991, 81)

As previously mentioned the Watson laboratory, as run by Eckert, put a great deal of value on academic freedom. He also wanted to avoid what he called “Projectitis”, the tendency of universities to create large projects to occupy their research staff. He felt it limited flexibility and freedom of researchers, especially those in the early stages of their career. As he put it: “We were going to be more like the university than the university is.” (Eckert MS 1967a, Part II, Tape 2, p. 20) Part of this policy was a high turn-over in staff and researchers at the laboratory. As a result a wide variety of projects were pursued. The freedom of the laboratory did not lead to a slack work environment. Grosch described how standard practice at the lab was to work into the night until all the machines had stopped working and then leave the machines for the IBM engineers to fix the next morning. He also described how all sorts of schemes for the use of computing were considered including calculating a betting scheme to win at horse racing. (Grosch 1991, 82, 88-89)

A sense of how Eckert guided his staff is suggested by the development of the IBM
610 Autopoint Computer, released as a product in 1957. This machine was developed at first by Lentz as the Personal Automatic Calculator, finished in 1954. Its function was similar to that of a modern programmable pocket calculator. It could perform arithmetic controlled directly by the operator, or carry out programmed instructions, with results displayed on a small cathode ray tube or printed out by an automatic typewriter. Since it used vacuum tubes and a magnetic drum memory the device was the size of a desk. (Bashe et al. 1986, 505-507) Lentz had been subtly encouraged to pursue this project by Eckert after some initial success. As Lentz put it in a later interview:

I know Eckert was very much interested in the idea of a personal computer and again, he was a very subtle man. I’m not sure whether I said to him, let’s make a personal calculator out of this or whether he said to me, why don’t you. (Lentz MS 1980, 15)

Eckert imported his hands off approach from academia but still managed to get results in terms of research papers and machines.

The subtly of Eckert’s management style had its detractors. One student of Eckert’s at Columbia and later Watson lab researcher, Joseph F. Traub (1932-), remembered Eckert as “totally disinterested in teaching” to the point that he seemed “burnt out.” (Traub MS 1984, 16-17) Eckert’s manner of speech has been described as so soft as to be almost inaudible. Gutzwiller felt that Eckert must have been more assertive in his earlier days to get as much from IBM as he did. Smith described Eckert’s manner as the opposite of Grosch’s outspoken style. Grosch for his part, while respectful of Eckert’s scientific accomplishments, was dissatisfied with Eckert’s lack of ambition to expand his part in IBM and the new opportunities in computing. This dissatisfaction led Grosch to seek opportunities elsewhere at IBM. (Rodgers 1969, 141; Gutzwiller MS 2007; Smith MS 2007; Grosch 1991, 119, 129)

Eckert’s endeavours in machine computation seem to have been more means to an end, astronomical calculation, and out of a sense of obligation to his benefactors at IBM. This would be consistent with his lack of ambition to move up at IBM. It would also
explain his initial intense efforts to promote machine methods that dropped off as such methods became ubiquitous. Eckert could then rely on others to develop the machines he needed and no longer was in a special position to promote the methods. This was one reason Eckert never became a giant of the computer field or the computer industry.

4.6 Eckert’s assistants at IBM

As has already been noted Eckert rarely did his machine calculations alone or directly. The skills of Hausman (Feinstein) have been mentioned. Rebecca (Becky) B. Jones was another assistant with whom Eckert wrote several papers and the book *Faster, Faster*. Unlike other assistants, she did little machine work and seems to have helped Eckert with organizational and analytical work. She is a rare example of an assistant who had a degree and research experience in astronomy.

Jones received an A.B. from Mount Holyoke College, Massachusetts, in 1927. After her graduation she worked as a research assistant at the Lick Observatory in California for four years, and spent three years as an instructor in astronomy at Wheaton College.\(^6\) In the period 1934-1935 she was the Pickering Fellow at the Harvard Observatory, taking courses and engaging in astrophysical research, her research included stellar spectroscopy, the changes in luminosity of a variable star and a statistical investigation of photographic plates showing spiral nebulae (galaxies). She would continue to study galaxies as an assistant at the observatory for several years and in 1940 coauthored a paper with the director of the Harvard Observatory, Harlow Shapely, on the distribution of galaxies. During the war she served with the US Navy as part of Women Accepted for Volunteer Emergency Service (WAVES). Jones would work for the Watson Lab from 1946-1958. The year of her departure was also the year of her marriage to Boris Karpov. She died

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\(^6\)There were two Wheaton Colleges in the United States at the time, a woman’s college in Massachusetts and a co-educational evangelical college in Illinois. I have not been able to confirm, which of these two she taught at.
Eckert had several other assistants, mostly female, including his student Harry F. Smith and his wife Dorothy Eckert. Smith noted he seemed to have a knack for finding “girls” to do the work he wanted to get done and Gutzwiller had a similar impression of Eckert surrounding himself with woman computers. (Smith MS 2007; Gutzwiller MS 2007) In fact Eckert’s assistants usually started out as competent machine operators who picked up the necessary skills in mathematical astronomy as they did their work for Eckert.

One reason that most of his assistants were female is simply due to the way that women often found work as human computers, including the wives of scientists. It was a low status profession even in cases where the practitioners were skilled. (Grier 2005, 276, 262) However not all of Eckert’s assistants were women. Smith himself was a male staff member at the Watson lab who became Eckert’s student. Although, Smith would assist with one of Eckert’s greatest triumphs in lunar theory, Smith would not make a career out of astronomical work. The skill set he required and learned to do the work for Eckert’s project was narrower than that. His research focused on applied mathematics and the emerging discipline of computer science. (Smith MS 2007) An interesting reversal of Eckert’s normal training regime was Jack Belzer who worked at the NAO during the Second World War. Belzer was already a computer at the office familiar with the calculations done there. Therefore Eckert had Belzer trained in the use of IBM machines so that he could supervise the work with the new machines. (Eckert MS 1942)

Like Jones, Dorothy Eckert had training and publications in stellar spectroscopy. Her maiden name was Dorothy Applegate and she graduated from Whitman College in Walla Walla, Washington in 1924 with a Bachelor’s degree. She was an assistant at the Lick Observatory in 1924-1926. Her official work consisted of routine computing work, but
she was able to arrange with researchers at the observatory to carry out observations and published at least two papers on spectrography. She was later a summer researcher at the Lick Observatory in 1929, while working as an instructor at her alma mater Whitman College. During the 1930s she worked as a home study instructor for Columbia. She would coauthor two papers with Eckert in the 1960s and this work will be discussed in Chapter 6.7 (Lankford 1997, 340-341; Wright and Applegate 1926; Applegate 1927; Personal Notes 1928; Catalogue 1933, 110)

Eckert’s assistants are unfortunately not as well detailed or remembered by history as Eckert himself. However, their presence as coauthors, in acknowledgement pages and so on shows how Eckert’s achievements were dependent on a team of calculators who worked under him. The fact that most of his assistants were female, while Eckert and most of his more noted colleagues were male, shows the strong sexual division of labour at work in astronomy and computer work during his lifetime.

4.7 Eckert as scientific advisor to IBM

In addition to Eckert’s role organizing scientific research at the Watson Lab he had a wider role in the doing of science at IBM. In some cases he acted as a special expert. When IBM received the contract for the navigation system for the B-52 Bomber, Eckert consulted with the designers on the basic issues of aerial navigation. (Kennard MS 1980, 25) Eckert also acted as a general consultant on the future direction of research at IBM.

Although Eckert had started out as the only research scientist at IBM, this soon changed. First Eckert hired several fellow scientists such as Grosch and Thomas to work at the Watson Lab. Also with the growing interest on the part of well funded scientists in IBM machines, it was decided that someone was needed to work on servicing the new

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7Dorothy Eckert was apparently a defender of her husband’s reputation and felt that he had received to little credit for his pioneering work. This may explain her decision to donate her husband’s papers to the Charles Babbage Institute in 1983. Before this, she also gave an interview to Henry Tropp and went over her husband’s papers with him. (Gutzwiller MS 2007; CBI 9 MS no date; Tropp 1978, 130)
scientific clients who had far different needs than the previous business clients. As a result Cuthbert Hurd (1911-1996), a scientist at Atomic Energy Commission, who had experience with punched card machines, was hired in March of 1949 in anticipation of the introduction of the IBM Card-Programmed Electronic Calculator (CPC). The CPC consisted of an arrangement of IBM accounting machines centred around the electronic IBM 604 connected to the latest IBM tabulator, IBM 402 Accounting Machine, for alphanumeric printing. Although not as flexible as a true computer, the CPC could perform long programs controlled via punch card and the set-up of a complex plugboard. It could also store a few dozen digits in a special relay storage. In 1949 the IBM research forum, an ongoing series of conferences on IBM techniques, was organized by Hurd and focused on the CPC. Due in part to his successful work promoting the CPC a new Department of Applied Sciences was created and Hurd was made the head of it. Soon Hurd and his staff were taking over duties from the Department of Pure Science. (Bashe et al. 1986, 84-86)

In 1954 senior management at IBM began to look for ways to rationalize their research and development. A task force made up of various people including NORC designer Byron Havens gave recommendations on the best way to do this. They decided to create a research organization separate from the final development of products. This decision was at odds with the tradition at IBM of engineer inventors trained on the job and reflected the proliferation of academically trained engineers and scientists at the company. This change was spurred in part by the rise of electronics. The new applied research division required a person to lead it. In a meeting that included Eckert, Hurd and a few other senior researchers and executives, the criterion for the position were decided upon. He would be a scientist with experience in administration and preferably not older than 45. In the end physicist Emanuel Ruben Piore (1908-2000) was hired as the new director of research. He had experience as an administrator at the Office of Naval Research and also a brief stint as a vice president in the private sector. (Bashe et al. 1986, 544-550)
Despite these displacements, the Watson Lab at Columbia would continue to grow with Eckert at the helm. In 1951 he received approval to triple the size of the lab and research in solid state physics became a focus of many in the lab. Some controversy arose about how the lab’s research efforts should be integrated with the rest of the company. Eckert saw the university environment as critical and did not see a need for physical connections between the Watson lab and the rest of IBM. The laboratory would continue to grow into the 60s. In 1962, when Columbia University gained its own computing lab, the computing machine portion of the Watson lab was closed and “Computing” was removed from the Watson Lab’s name. In 1964, a project in molecular biology was added to the laboratory’s mandate. In 1966 Eckert finally stepped down as head of the laboratory, he officially retired from the lab in 1967 and he retired from the University in 1970. The Thomas J. Watson Laboratory at Columbia University closed in 1970. (Brennan 1971, 32-35, 47-51; Bashe et al. 1986, 530-533)

Wallace Eckert was honoured for his achievements by various bodies. In 1966 the National Academy of Science awarded him the James Craig Watson Medal for contributions to the science of astronomy. The medal was given ”For his pioneering contributions to scientific computing and to the theory of the motion of the moon.” In 1967 Eckert was made an IBM Fellow allowing him to pursue research at IBM. He also received an IBM Outstanding Contribution Award in 1969. He also received an honorary doctorate in 1968 from his alma mater Oberlin College. (Tropp 1978, 129; Gutzwiller 1999, 160; National Academy of Science MS no date)

Wallace Eckert was at the forefront of a change in IBM’s culture from a company of salesman and self-taught engineers and inventors into a much more technically and academically savvy company, at the cutting edge of science and technology. Eckert himself was an advocate for a very traditional academic way of doing research and to an extent this was adopted by IBM. Ultimately, Eckert’s dedication to his own astronomical research limited his engagement in the transformation of IBM to service science. The
commercialization of IBM’s scientific computing potential may have begun with Eckert’s work, but it was others like Cuthbert Hurd who would systematically seek out new scientific customers for IBM’s machines and so remake the basis of IBM’s business. The particulars of Eckert’s scientific research during his time at IBM is the focus of Chapters 5 and 6.

Eckert’s academic style and goals sometimes conflicted with the corporate style of IBM. In particular he had conflicts with its formidable president Thomas J. Watson Sr., on such things as the use of IBM time clocks by employees. Yet he clearly maintained an appreciation of the engines of industry that made his scientific developments successful. In a letter to the New York Times he summed up his conflicts with Watson Sr. with the comment: “In summary, I wouldn’t have missed it.” (Eckert 1969) This seems to sum up his attitude to his entire 22 years as an IBM employee.
Chapter 5

Eckert’s Work on the SSEC

Eckert’s role in designing and constructing the SSEC extended to deciding what its first assigned problem would be. For this “dedication” problem he chose a subject near to his own heart, lunar theory. He also arranged for one other problem to be solved on the SSEC, the positions of the outer planets from 1653 to 2060. He carried out this problem in collaboration with Dirk Brouwer of Yale and Gerald M. Clemence (1908-1974) of the Nautical Almanac Office, two men with whom he had collaborated, both before and during the war. Both of these problems would yield voluminous results that would set new standards in accuracy and would stand as reference works for decades.

5.1 Direct Calculation of Brown’s Lunar Theory

The most famous problem Eckert tackled on the SSEC was the calculation of the position of the Moon for several times, directly from Brown’s theory. Eckert proposed this as the first or “dedication” problem for the SSEC as early as 1946. It was known that Brown’s Tables had approximated the solution, including the complete neglect of some terms and so did not achieve the full precision the theory would allow for. The intent of the calculation was to determine the extent of the error introduced in the Tables and to explore the possibility of calculating a new, more accurate, ephemeris for the moon.
Improvements in the ability to measure the Moon’s position were anticipated that would make a more accurate lunar theory necessary. (Eckert et al. 1954, 283)

The initial analysis of the problem was done by Eckert together with Rebecca Jones and then the problem was converted to a machine form by H. K. Clark with the assistance of W. F. McClelland and Aetna K. Womble. Sometime after the initial set of positions were calculated, the problem was recoded in line with a better appreciation of the operation of the SSEC. John W. Backus, D. A. Quarles Jr. and Womble did the work of recoding the problem. (Eckert et al. 1954, 285)

After the calculation of 7 check dates, the first part of the calculation consisted of calculating 60 positions for the Moon, at 12 hour intervals for the 30 day period from April 24th to May 24th. In principle this required the calculation and addition in proper order of a little more than 1650 trigonometric terms. The equations containing these codes could have been transcribed into the machine and calculated in a completely straightforward way. However, the procedure was further streamlined by Eckert and his team to minimize the amount of arithmetic. The terms were divided into three groups for the purposes of computation: the 17 fundamental arguments, 50 additive terms and about 1600 periodic terms. (Eckert et al. 1954, 283-292, 353)

The fundamental arguments consisted of the mean values of the Moon’s longitude, the mean longitude of the Moon’s perigee, the mean longitude of the Sun, the mean longitude of the Sun’s perigee and four terms which were simple differences between the lunar and solar terms (l, l’, F and D). In addition to the basic terms, there were six planetary terms for incorporating the perturbations of the six inner most planets (Mercury, Venus, Earth, Mars, Jupiter, Saturn) and another value for the mean longitude of the Moon for use with the planetary terms. The four differences (l, l’, F, D), and sometimes the planetary terms in various combinations, gave the arguments of all the periodic terms found in the equations. So each periodic term was of the form $K \sin(ml + nl' + oF + pD...)$, where m, n, o and p are integers. The arguments for the lunar terms and the four differences
between lunar and solar terms were calculated from 3rd order polynomial functions of the time (ie $a+bt+ct^2+dt^3$). The arguments of the solar terms were calculated by second order polynomials. The arguments for the planetary terms and the associated longitude of the Moon were calculated from a simple linear relationship with the time (ie $a+bt$). The constants of the terms ($a$, $b$, $c$ and $d$) were given by various combinations of the parameters of the Moon’s motion. The parameters were the constant of eccentricity of the Moon’s orbit, the constant of inclination of the Moon’s orbit, the constant of lunar parallax (mean lunar parallax) and the eccentricity of the Sun’s orbit. These constants were left in literal form during Brown’s derivation of his equations, allowing the effect of changing their values to be directly obtained from the equations. The empirical value of the ratio between the mean motions of the Moon and Sun was entered directly during the development of the equations and so is only present implicitly in the calculation. (Eckert et al. 1954, 286-288)

The additive terms further modified the mean longitude of the Moon, longitude of lunar perigee, longitude of lunar node, the planetary terms for the Earth, Jupiter and Saturn, $l'$ (one of the differences between the Solar and lunar terms) and the constant of inclination of the Moon’s orbit. Brown’s method defined the additive terms as linear combinations of the unmodified fundamental arguments, but in the actual computation the arguments were calculated by use of abbreviated polynomials in time, usually a linear expression. Some terms included the square term in their argument. The effects of the additive terms was small enough that in some cases, such as many calculations involving the inclination of the moon, Brown neglected the effects in the numerical values used in the Tables. The computations on the SSEC also carried the corrections of the additive terms and other corrections through in many more cases. (Eckert et al. 1954, 285-289, 344-345)

The periodic terms were the over 1600 trigonometric terms, with arguments derived from the fundamental arguments by linear combination, that are summed to give the
coordinates of the Moon: longitude, latitude and parallax (sine parallax). The form of these equations are as follows:

\[
\text{Longitude} = L(i\theta) + \delta L(i\eta) + \delta L(i\alpha) + \delta L(i\delta)
\]

\[
\text{Latitude} = (1 + C)[\gamma_1 \sin(S) + \gamma_2 \sin(3S) + \gamma_3 \sin(5S)] + \delta \text{Lat.}(i\epsilon)
\]

\[
\text{Parallax} = \delta \sin\Pi(i\gamma) + \delta \sin\Pi(i\zeta) + \frac{1}{6}[\delta \sin\Pi(i\gamma) + \delta \sin\Pi(i\zeta)]^3
\]

where \(S = F(i\theta) + \delta L(i\eta) - \delta \Omega(i\eta) + \delta S(i\beta)\), all the terms noted \(i\) along with a Greek character refer to lists of terms, the “functions” surrounding them indicate the summation of all or some subset of those lists. \(i\alpha, i\beta, i\gamma\) contain respectively the solar terms in longitude, latitude and sine parallax. The list \(i\beta\) is divided further into those contributing to \(S, \gamma_1 C, N\) and the coefficients \(\gamma_1, \gamma_2\) and \(\gamma_3\). \(i\delta, i\epsilon\) and \(i\gamma\) contain respectively the planetary terms in longitude, latitude and sine parallax. List \(i\eta\) is the list of additive terms and \(i\theta\) refers to the fundamental arguments. So \(L(i\theta)\) refers to the fundamental argument for the mean longitude of the Moon, \(\delta L(i\alpha)\) refers to the summation of all the solar terms that modify the mean longitude, \(N(i\beta)\) refers to the sum of a subset of the solar terms that contribute to the latitude and so on. Note that while \(\sin(S)\) refers to an actual sine function, \(\sin\Pi\) is simply short hand for sine parallax and indicates the summation of lists of terms contributing to it. (Eckert et al. 1954, 288-289, 344)

If calculated to their maximum precision, each term would have the form \(K \sin(a+bt+ct^2+dt^3)\) and, in principle, Eckert and his associates could have simply calculated all the terms in this form and summed the results as indicated. However, Eckert preferred an approach that minimized the number of computations. In practice the majority of the terms, 941, were calculated via a first order approximation of the form \(K \sin(a+bt)\). Thanks to the various shortcuts the 941 small terms were added to the expressions in 4 and a half minutes. (Eckert et al. 1954, 347-349)

Of the remaining approximately 700 terms, Eckert and his team employed various methods to expedite calculation, such as taking advantage of trigonometric identities and
the relationship between the arguments of the terms. First they evaluated and stored the sines and cosines of the fundamental arguments for a given time and various combinations. When calculating the larger periodic terms they evaluated the sines and cosines by multiplication and division of these calculated values. So for example a periodic term of the form $A \cos(l - 2D)$ would have been evaluated as $A[\cos(l)\cos(2D) + \sin(l)\sin(2D)]$ with each individual trigonometric term $\cos(2D)$ already calculated and in storage. In this way the values for over 600 terms were found based on 80 precalculated combinations.
The complete calculation of a single position took approximately 7 minutes. (Eckert et al. 1954, 347-350; Eckert 1948b, 322)

5.2 Features of the SSEC Displayed in the Lunar Problem

The mechanics of the calculation illustrate some interesting examples of continuity and change in Eckert’s work. For example, sines and cosines were calculated in three different ways. For the small terms, first a sine table of 100 values at equal intervals was read into relay memory by the high speed table look-up and then the value needed for the sine of a given term was interpolated using a function of the form: $\sin(x) = A + Bn$ where $A$, $B$, and $n$ are four digit numbers.\(^1\)

Rather than carry out a separate multiplication and addition the operation was performed using a single multiplication. The multiplication took the product of two numbers each of 14 digits: the first 4 digits of $A$ followed by 6 zeroes and then the 4 digits of $B$ (A000000B) and the second $n$’s four digits, 9 zeroes and a 1 (n0000000001). So for $A=1234$, $B=5678$ and $n=9012$ the first number would be 12340000005678 and the second 90120000000001. Since the SSEC calculated products to 28 digits before truncating

\(^1\)Cosines were calculated by adding a quarter revolution, equivalent to 90°, to the argument and treating them as sines.
them for storage, the resultant product had the form \( A\text{n}00...00(A+Bn)000000B \) and the central digits were selected to be stored giving the interpolation, \( A+Bn \).\(^2\) Eckert had detailed an analogous method in his 1940 book for punched card machines. The use of this procedure in the SSEC shows the persistence of earlier machine calculation techniques in the new electronic machines despite their prodigious speed. (Eckert 1940a, 29-30; Eckert et al. 1954, 348)

The calculation of the small terms was also significant as an example of the use of the ability of the SSEC to modify its instructions. The appropriate sine term from the table in relay memory was found by modifying the instruction to retrieve it. The values were entered into the tables so that the argument of the sine function (\( x \)) had a calculable relation with the address number of the relay storage. The instruction to be modified left the address to retrieve the sin value from as 0 and the manipulation consisted of adding the address value into the original instruction. This is perhaps the earliest recorded use of the stored program capability in an actual machine on an actual problem. (Phelps 1980, 261)

The other sines and cosines were calculated in two ways. During the calculation of the initial 67 positions, sine and cosine were evaluated to a high precision by reference to a table stored on punched tape. In all later computations a series solution was used to calculate values of the sine and cosine instead. This change was part of a recoding of the

\(^2\)For an understanding of how this effect is achieved consider the following long multiplication of two 4-digit numbers:

\[
\begin{array}{l}
A00B \\
\times N001\
\hline
A00B \\
0000 \\
0000 \\
\hline
CD0FG \\
CD0EH00B
\end{array}
\]

where \( A, B, C, D, E, F, G, H, N \) stand for various individual digits. Let \( A = A, B = B \) and \( n = N \) then \( FG = Bn \) and \( EH = A+Bn \). Thus by selecting the two middle digits (\( EH \)) an addition and multiplication is turned into a single multiplication and this can be scaled up to higher numbers of digits as in the SSEC problem.
problem after its initial run (the dedication problem of January 1948). Although other changes were also undertaken, this was the only one described by Eckert or any other account I have found. Presumably the changes were made to increase the efficiency of the calculation, avoiding processes that took longer or were prone to faults. The change suggests that Eckert and the other designers of the SSEC may have overestimated the utility of tables as a short cut for calculation. The change from a tabular to a series method of obtaining values for functions shows how working with the SSEC changed ideas about machine calculation. (Eckert et al. 1954, 284) The methods used were in a state of flux.

In fact the issues of what methods were practical for an automatic computer had a long history. Charles Babbage speculated on the usefulness machine readable tables for his analytical engine. Babbage suggested that his analytical engine would calculate fast enough that reading from tables would be unnecessary. Later electronic machines would not emphasize the use of tables as the SSEC did. Although, basic math processors often use built in tables to speed up certain common calculations such as finding a sine value.\(^3\) (Babbage 1961b, 59)

One reason for the emphasis on using tables on the SSEC was the use of punched card tables by Eckert, Comrie and others in the 1930s and 40s work with more limited punched card machines. However, with the SSEC the logistics of handling data (searching for, inputting and outputting numbers) had changed drastically from earlier punched card machines. Of punched card machines produced in the 1930s, sorting machines were the fastest punched card machines, while multipliers were slower by a large margin. Also, because they only performed one or two operations, iterating a complex algorithm could take several passes of a card involving a great deal of operator involvement. On the SSEC calling up any data, much less the searching a table, took as long or longer than

\(^3\)An extreme example of tables used by a later machines for basic math processing is the IBM 1620, released in 1959, which was nicknamed CADET (Can’t Add Doesn’t Even Try), because its design replaced arithmetic circuitry with a small and cheap table-look-up. (Bashe et al. 1986, 508-510)
multiplication. Also arranging a series of calculations was straightforward on the SSEC.

Although not explicitly stated, the table of sines used for large terms must have been subject to interpolation, since being composed at so fine an interval would have made storing the tables and searching through them inefficient. Also, Eckert and the other experts in computation on the project came from a tradition where it was natural practice to interpolate tabular values in actual computations. They spent considerable time and energy on schemes for finding the optimal intervals of tables for interpolating from, while maintaining precision. Grosch recounts how his part in the opening of the SSEC was the creation of a method to minimize the size of such tables. Grosch also explains that this method was much used in IBM’s early CPC, before large internal memory made it unnecessary. (Grosch 1991, 8)

In order to check for machine error, the calculation was run a second time in parallel with the first and results were checked against each other for discrepancies at fixed intervals. The machine would stop if an error was detected. In order to give some independence to this error checking, the check calculations were done using equivalent but numerically different angles. Even in hand computation running parallel sets of identical hand computations was not seen as the best means of checking for computational error by such experts as L. J. Comrie. In the computer age, this method of error detection would become seen as cumbersome and inefficient in the light of error detection schemes already implemented on Bell’s Mark V calculator and that would soon become standard in large computing machines including the IBM NORC. These schemes encoded redundant information, a check digit or check bits, with the numbers and detected mismatches between the number and its check digit, caused by mechanical failure.4 Such a system

4The NORC counted the number of bits in the 1 or “on” position in a word and recorded a two-bit number (zero to three) that was equal to the remainder from the division of the number of “on” bits by four. The Mark V implemented a different error detection scheme. Digits were represented in a seven-bit bi-quinary format. Two bits represented either 0 or 5 with one or the other bit in the “on” position and the other in the “off”. The other five bits represented numbers between 0 and 4 by turning on only one of the bits in that group. In this way the absence of any “on” bits or the presence of two or more “on” bits in the same group indicated a mechanical failure. (Ceruzzi 1990, 212; Eckert and Jones 1954, 13-14)
does not detect every error and would usually be further supplemented. (Eckert et al. 1954, 348; Comrie 1928b, 509; Eckert and Jones 1954, 13-14)

5.3 Comparison of the SSEC Results to Brown’s Tables

An initial comparison of the 60 positions against values generated using Brown’s Tables was carried out by Gerald Clemence and Edgar W. Woolard of the Naval Observatory. Rather than use existing ephemeris tables generated from Brown’s Tables, Woolard and Clemence recalculated the values in duplicate. The calculation allowed, among other things, for the empirical term of Brown’s equations to be replaced and greater precision in the numbers calculated. The “empirical term” was designed to compensate for a large residual inaccuracy in the equations unexplainable by gravitational theory. Subsequent work had shown this to be a result of the irregular period of the Earth’s rotation and new corrections were being developed to account for the effect. The effects were neglected altogether for the comparison and the new correction used in the subsequent ephemeris. Differences were expected between the tables and the SSEC calculations. As mentioned the tables neglected some small terms. On the basis of rounding errors alone a “probable error” of ±0″.02 in latitude and longitude and ±0″.0015 in the parallax were expected for the tables. Further differences due to the imprecision in the values extracted from Brown’s Tables by different human computers (using different interpolation rules etc.), suggested the error range might be as high as 0″.05 in longitude. (Sadler and Clemence 1954, vii-viii; Eckert et al. 1954, 284; Woolard 1954, 365-371)

The rationale for comparing the SSEC solution to the values found from Brown’s Tables rather than some other source is not made completely explicit. The Tables had

\footnote{The probable error is ±0.675 standard deviation units and is such that for normal distribution the probability of an error greater than this is 50%. (James and James 1992, 116)}
been used since 1919 and were known to give accurate values for the position of the Moon when compared with observation in a wide variety of circumstances, more accurate than any other known method. Therefore any large deviation between the values from the *Tables* and the SSEC calculation would tell against the accuracy of the SSEC calculation. Close agreement between the two values would guarantee that the SSEC calculation had attained at least the accuracy of the earlier *Tables*.

The initial comparisons showed differences in the parallax as great as $0''.011$, in longitude as great as $0''.104$ and in latitude as great as $0''.2$ (see figures 5.1, 5.2 and 5.3). The standard deviation between the values of the two calculations in parallax is $0''.005$, in longitude is $0''.05$ and in latitude is $0''.10$.

Woolard judged the differences in parallax as slight and not worthy of comment. However he found the discrepancies in longitude and latitude in need of further examination. The error in latitude was both larger than Woolard expected and strongly systematic, while the error in longitude he deemed of interest solely due to its possibly systematic character. On this basis he undertook a more detailed analysis of the causes of these differences. (Woolard 1954, 367-371)

In addition to the terms neglected in Brown’s *Tables*, the expression in the *Tables* also combined terms in an approximate way, different from the direct approach used by the SSEC. The effects of smaller elements would be accounted for indirectly by the tables, in particular in the cases of long period terms and certain corrections. In order to facilitate the detailed comparison, Eckert’s staff at the Watson lab provided Woolard with values for the individual terms making up the SSEC calculation at selected times. The SSEC calculation had not listed these values and so they were reconstructed using standard punch card machines. These values were then recombined to allow comparison with individual values from Brown’s *Tables*, although in some cases no exact equivalent

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6 The publications do not list the full set of values compared explicitly. Only 12 values are given for parallax. The full range of 60 positions is visible in the graphs for longitude and latitude, but explicit values are only given at 12 times for the latitude and 14 times for the longitude. The standard deviations above are calculated by me based only on the explicit values. Using the graph to estimate all sixty values for latitude and longitude suggests standard deviations of $0''.04$ in longitude and $0''.12$ in latitude.
Reconstructed graph of the difference between the value of parallax calculated by Brown’s *Tables* and by the SSEC, for 12 dates.

**Figure 5.1**

(Woolard 1954, 369)

Graph of the difference between the value of longitude calculated by Brown’s *Tables* and by the SSEC, for 60 times at half-day intervals.

**Figure 5.2**

(Woolard 1954, 369)
Graph of the difference between the value of latitude calculated by Brown’s *Tables* and by the SSEC, for 60 times at half-day intervals.

**Figure 5.3**

(Woolard 1954, 369)

would exist. (Eckert *et al.* 1954, 284; Woolard 1954, 371-374, 385)

Woolard’s analysis showed that the main contribution to the systematic discrepancy between SSEC and tabular values was found in three tables intended to approximate the effect of several, otherwise neglected, long period terms. Woolard suggested that the accumulation of rounding errors and approximations in the make-up of the three tables created the error. Woolard attributed one other source of the discrepancy in longitude to the secular variation in the Earth’s orbital eccentricity. (Woolard 1954, 385-386, 393, 398)

In the case of the latitude Woolard was able to uncover an actual oversight in the construction of the tables. The term $\delta \gamma$, representing the effects of long term variation in the constant of inclination of the Moon ($\gamma$), had been included twice. In order to confirm this as an error Woolard compared an 18.6 year period of observations of the Moon against the tables for a residual difference with the appropriate magnitude and period. Woolard found the discrepancy confirming the mistake in the calculation of
the tables. Woolard also identified some of the discrepancy as due to approximation and rounding errors in the efforts to incorporate the long period terms without direct calculation. Again he noted that the exact magnitude of the discrepancies were difficult to ascertain because of the indirect methods of calculation employed in Brown’s *Tables*. (Woolard 1954, 408-413, 415)

The discussions, by Woolard, Eckert and their colleagues, of the SSEC calculations’ accuracy focused almost exclusively on their agreement with Brown’s theory, the mathematical precision of the work, and not its physical accuracy. In terms of agreement with the theory they found the SSEC calculations to give the latitude and longitude to a precision of $0''.001$ and the parallax to $0''.0001$. Mention was made of the close confirmation of Brown’s theory by observation, meaning presumably checks of the theory’s detailed prediction at key dates, and this gave an indirect indication of the calculated positions’ expected accuracy. The SSEC went on to calculate 176 positions around the dates and times of eclipses, presumably to make comparison with the especially accurate eclipse measurements of lunar position, but these were not discussed. As mentioned Woolard showed that the double application of $\delta \gamma$ in Brown’s *Tables* created an observationally detectable systematic error. However, there was no discussion of the numerical value of the agreement between the SSEC’s results and observations. (Eckert *et al.* 1954, 283-284, 353; Woolard 1954, 364-367)

The failure to discuss the actual accuracy of the work is strange given the pains taken to demonstrate its precision. In fact both later theoretical work, by Eckert and others, as well as new observations made by radar, spacecraft and lasers, would demonstrate the inaccuracies in Brown’s theory were well in excess of its mathematical precision. For example, as part of preparation for unmanned and manned missions to the Moon, the Jet Propulsion Laboratory (JPL) created a series of ephemerides based on Brown’s lunar theory including corrections based on new observations and developments in theory. A comparison of one such ephemeris, with observations using the latest techniques and
telemetry from lunar vehicles, found discrepancies on the order of $0''.055$ in latitude and longitude (about 105 meters in absolute terms) and $0''.0018$ in parallax (about 200 meters). (Mulholland and Devine 1968, 874-875) While this is extraordinarily accurate, the inaccuracy was many times greater than the expected “agreement with theory” calculated by Woolard and Eckert.

Two reasons for the absence of discussion of the expected accuracy of Brown’s theory seem to be the limits of observational accuracy and the absence of an estimate for the error. One motivation for the SSEC work and the subsequent lunar ephemeris was that observational techniques were improving. On the other hand, the example of the systematic error found in Brown’s Tables suggests that existing observations could find errors if researchers knew what to look for, specifically the error’s periodic character. Still, the lack of emphasis on the potential limits of Brown’s theory suggests the confidence Eckert and his colleagues had in the power of theory and mathematics.

### 5.4 Compilation of the Improved Lunar Ephemeris

After the initial analysis, the coding method was changed as detailed earlier. In this new configuration 114 positions of the Moon were calculated around the dates of various eclipses. In 1951 a month’s worth of calculations (positions for 60 half-day intervals) were done for the United States Army Map Service for use in geodetic measurements.\(^7\) All of this work was done using Brown’s value for the mean longitude. (Eckert et al. 1954, 284, 353)

In 1952 the International Astronomical Union’s (IAU) Eighth Assembly adopted a resolution to change the method of calculating the ephemeris of the moon. The resolution specified removing Brown’s empirical term and replacing it with a new term to account

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\(^7\)Geodetic, from geodesy, literally refers to measuring the Earth. Geodetic measurements are used to create accurate maps, calibrate coordinate systems and otherwise accurately measure and represent the Earth’s topology.
for the variance created by the irregularities in the Earth’s rotation and the secular acceleration of the Moon due to the tidal interactions between Earth and Moon. The resolution also called for an increase of the precision of the published ephemeris to 0’.01 in latitude and longitude and 0”.001 in parallax. The recalculation also related to the definition of ephemeris time. The irregularity of the Earth’s rotation meant that the definition of time based on the solar day introduced deviations in the motion of heavenly bodies from theory. Ephemeris time took the solar year as the basic unit by which to define time. However the most accurate way to measure ephemeris time would be with the position of the moon, because of the accuracy of the theory of its orbit that relates the position to the time. (Sadler and Clemence 1954, vii-viii)

The source of the values for the new more precise ephemeris would be provided by Eckert. The work done so far had established the precision and accuracy possible using the SSEC. The Improved Lunar Ephemeris 1952-1959 (ILE), published in 1954, was based on SSEC calculations using the new constants and value of mean longitude, and gave positions at half-day intervals. In fact Eckert and his team had completed an ephemeris for the period from 1952 to the end of 1971 and this larger unpublished set of values is sometimes referred to as the Improved Lunar Ephemeris or ILE. Starting in 1960 the various national offices tasked with producing lunar tables would use the values of the 20 year ephemeris, until then the Improved Lunar Ephemeris would be available. However, the SSEC did not calculate all the positions in the ephemeris. Instead, detailed calculations were done on the SSEC at 100 day intervals and the half-daily intervals were interpolated. The staff of the Watson lab carried out the interpolation using a variety of machines at various stages including the Pluggable Sequence Relay calculators, IBM 604, SSEC and the IBM 701. (Eckert et al. 1954, 352-356)

The interpolation was carried out on a term by term basis in four groups. Eckert and his team interpolated using 1047 terms to intervals of 2 days using trigonometric
methods of the form:

\[ K \sin(x \pm h) = K \sin(x) \cos(h) \pm K \cos(x) \sin(h) \]

\[ K \cos(x \pm h) = K \cos(x) \sin(h) \mp K \sin(x) \cos(h) \]

They did not interpolate from individual terms to individual terms, but rather from one time value of the key sums (such as \( N(i\beta) \)) to another using the individual terms. A further 286 terms were interpolated using trigonometric terms to one day intervals and the remainder (260 terms) were interpolated to the half-day values using the trigonometric method. After the initial interpolation those values not at half-day intervals were interpolated further using a six point Lagrangian formula for the periodic terms. This procedure has the form:

\[
f(x + 0.5h) = \frac{1}{256} \left[ 3f(x - 2h) - 25f(x - 1h) + 150f(x - h) + 150f(x) \\
- 25f(x + 1h) + 3f(x + 2h) \right]
\]

where \( f(x) \) is the function to be interpolated and \( h \) the interval to which the function had already been calculated (in this case 2 days and 1 day). They supplemented the interpolation by use of the sixth differences of the functions to modify and also check the values. They calculated the fundamental arguments and the additive terms using a Bessel interpolation (see Chapter 3 of this thesis for details of that method). They made a final error check of the total sums using the eighth differences. (Eckert et al. 1954, 353-358; Comrie 1928b, 507)

The use of Lagrange interpolation and Bessel interpolation is noteworthy because of Comrie’s early commentary on it. Comrie had suggested in 1928 that the Lagrangian and Bessel interpolations could be shown to be equivalent algebraically, but that the method of difference was easier to routinize, more suited to machine computation and the differences thus calculated allowed an easy check on error. Although others advocated checking the Lagrange technique by the differences, Comrie felt that in practice it
would be to easy to neglect this extra step. Apparently the speed and flexibility of relay and electronic calculators, which allowed quick multiplications and the automatic performance of several steps of a computation, altered the logistics of computation. (Comrie 1928b, 507-509) On the other hand the continued use of these techniques of interpolation shows the continuity between earlier traditions and the new developments in electronic computation.

The work on the interpolations used resources beyond those at the Watson laboratory. The actual machine work involved was organized by David Mace with the assistance of Ann F. Beach, S. C. Nicholson and R. T. Mertz. In addition to the Watson lab machines they made extensive use of the IBM 604 at the New York Service Bureau of IBM. Seeber was responsible for the calculations on the IBM 701. This same IBM 701 replaced the SSEC at IBM headquarters and operated under the IBM Technical Computing Service rather than under the auspices of the Watson lab. (Eckert et al. 1954, 285)

The machine work began with the SSEC’s calculation of the key dates: values of position at intervals of 100 days between the end of 1951 and the beginning of 1972. The output of these calculations included not only the final values of position, but also the values of a large portion of the individual terms [of the form \(\sin(x)\) or \(\cos(x)\)] to be interpolated from. The majority of the terms were calculated using the IBM 604 and the relay calculators to the approximation \(\sin(a+bt)\) and using a punched table of sines. The values of the interpolating trigonometric functions [\(\sin(h), \cos(h), \sin(2h)\) etc.] were also prepared by the IBM 604 and relay calculators. These values were punched on cards to 5 digits for the values of the functions at key dates and to 4 digits for the interpolation factors \(K \sin(h)\) etc.. Half the cards prepared contained the value of terms for three key dates and the necessary factors to interpolate to 3 new dates, the sums of the interpolates of each date were recorded on a new card. The other half of the prepared cards were of the same form, except they replaced the third interpolation factor for each term with the sum of the previous five interpolations factors, the sum of the product of these terms and
the functions at key dates would then act as a check against the sum of the previous five dates. In total about 2,700,000 cards were run through the machines for a total of about 8,000,000 products. Evaluation occurred at about a rate of 18,000 products an hour (5 per second) and checks were performed once an hour. 558 terms in Longitude were interpolated on the SSEC near the end of its life in 1952. (Eckert et al. 1954, 356-357)

81 terms could not be computed to the lower precision used in the previous operation. These terms were interpolated on the IBM 701. In addition to calculating the terms to greater precision, many of these terms were also subject to corrections that were ignored for smaller terms. The IBM 701 took in 30,000 numbers on 5,000 cards as input and outputted 60,000 on 12,000 cards in the space of 17 hours. (Eckert et al. 1954, 357)

5.5 Implications and Impact of Improved Lunar Ephemeris Calculations

The common misconception that the SSEC alone calculated the tables of the Improved Lunar Ephemeris belies the complexity of the undertaking. The actual details of the calculation suggest several things, such as the complex economics of computing. At a cost of $300 an hour the SSEC was a very expensive way to multiply numbers together, whereas the IBM 604 costing on the order of $550 a month would provide multiplication at 1/10th the speed and to a lower precision, but also more cheaply. Grosch coined his eponymous law in an attempt to encapsulate the economics of computing in these early days. Grosch’s law states that cost of a computation, measured by the number of multiplications, decreases as a factor of the square root of the increase in speed or: “to do a calculation ten times as cheaply you must do it one hundred times as fast.” (Grosch 1953, 310)

Grosch’s law suggested that larger and faster machines should offer an economy of scale. However, it also warned that costs decrease less than speed increases. Also, Grosch
reasoned based on the assumption that the machine would be continuously in use with no idle time. This fact helped explain why it could still be cheaper to use smaller machines. For example, Grosch associated his decision with using multiplication as the measure of speed with Comrie’s comments about the economics of computing. The context was operations using a punched card tabulator and only infrequently requiring multiplication. Comrie reported it was cheaper to have human operators perform the multiplication on desk machines than to hire an expensive punched card multiplying machine that would be little used. (Weik 1961, 332-336; Grosch 1991, 131-132)

Another thing the computation of the lunar ephemeris on the SSEC illustrated was the continuity between Eckert’s earlier work and that using electronic machines, not only in terms of content but also in the details of computation. These include both interpolation methods and specific tricks to speed up arithmetic based on the features of the machine. Finally, Eckert’s continued commitment to this sort of astronomical work also meant continued associations with people at the Nautical Almanac Office, like Clemence.

The ILE would remain the benchmark for accuracy in lunar position into the 1960s and specifically the basis for the standard astronomical and navigational tables published by the US and British Almanac offices. As previously mentioned, the JPL, working as part of the American drive to land on the Moon, developed more accurate models at first based on Brown and Eckert’s work. In this work they enlisted the aid of Eckert in various ways, most of which will be the concern of the next chapter. One incident however illustrates the extent to which Eckert’s work remained the standard for at least a decade.

In 1960 a request came to Eckert from JPL research engineer Neil Block, who requested any flow charts, symbolic representations or actual programs that might exist for the IBM 701 work mentioned in the Ephemeris. Block was particularly interested in the calculation of an array of sines and cosine terms. Block also asked if any programs
had been produced to do the calculations of lunar position on IBM’s later large scientific machines such as the IBM 704. Block also stated his intention to rederive Brown’s theory with new adopted values for the constants. Presumably Block intended to reproduce Eckert’s method of calculating lunar positions and use it as a check of any improved lunar theory. Eckert explained that the computation in question had not been done on the IBM 701 and that there was no plan, at the Watson lab, to extend the lunar ephemeris work to other machines. Instead the British Almanac Office had taken on the task of calculating the next ephemeris. Eckert noted that he and others were working on improving lunar theory, but they had no results to report so far. Eckert also explained that the SSEC differed substantially from the IBM 701 and other machines and so the best record of the computation plan for the work was contained in the paper describing the computation. (Block MS 1960; Eckert MS 1960)

The exchange shows how Eckert’s ILE remained the standard for accuracy into the 1960s despite advances in computer technology. It also demonstrates the beginning of a relationship between Eckert and the JPL that would continue throughout the 1960s. The exchange also gives an interesting example of the speed of development in the early days of computing. Block’s request suggests that, within a decade, a practice of sharing and reusing code had become established in some parts of the scientific community. This contrasts with Eckert’s and the SSEC team’s work that appears to have been a series of one-of-a-kind projects. On the other hand, Eckert’s efforts in the 40s to enable the sharing of libraries of punched card tables with physical and mathematical constants punched on them, represents a similar effort to share previous work to the benefit of all and one that took advantage of the fidelity of digital reproduction.

The reuse of code was pioneered by several early computer pioneers in the 1940s. The first kind of computer code to be widely shared were subroutine libraries. The concept of the subroutine was first developed by von Neumann and Goldstine at the Moore school. However, the first subroutine library was created at Cambridge by Maurice
Wilkes (1913-) and the team responsible for the EDSAC. Sharing of subroutine libraries occurred from a relatively early date. The experience of University of Toronto with its Ferranti Mark I (Ferut) is illustrative. University of Toronto purchased the Ferut in February of 1951. Calvin Gotlieb (1921-), of the University of Toronto computation lab, went to Manchester in the same year, where the prototype for the Ferranti machine was developed, for training in the use of the Ferranti machine. While at Manchester he obtained an entire subroutine library. (Campbell-Kelly and Aspray 2004, 167-168; Campbell 2006, 147) As computers and their programs increased in complexity, the code making up individual programs necessarily became distributed more and more widely and the work of more diverse hands.

In a scientific context, the sharing of code also plays a role as a way of independently establishing results. Although replication is most commonly thought of as a feature of experiment, it is equally important in computational matters. Of course many computations are easily reproduced making replication trivial. However, the computations of celestial mechanics were sufficiently complex that their reproduction was not always trivial. In 1959 Eckert asked Woolard about some apparent inconsistency in Brown’s derivation of the value of a term, this led Woolard to remark: “I encountered more of these slight discrepancies and obscurities than I succeeded in clearing up.” (Woolard MS 1959) The fixation of a computation into machine code, and the ability to quickly and easily reproduce such code, meant that the electronic computer created the potential for greater sharing of the specific details of computation.

Within a year of his 1960 exchange with Eckert, Block and his group at the JPL had taken over the computation of future lunar positions on the basis of the ILE from a group at Her Majesty’s Nautical Almanac Office in Britain. When the superintendent of the office, D. H. Sadler (1908-1987) and his assistant, G. A. Wilkins (c.1930-), accepted the arrangement, they also requested that an annotated copy of the code or a FORTRAN version of the program, flowcharts and initial data be shared by the JPL with the office.
This was done to ensure the office and the JPL agreed on interpretation of the problem and, if necessary, to allow the office to do their own check run. This illustrates the extent to which the sharing and reuse of code had become common practice in scientific computation. Wilkins also communicated the errors they had found in the ILE. With this understanding in place the JPL began its work in calculating ephemerides. (Sadler *MS* 1961; Wilkins *MS* 1961) The Jet Propulsion Laboratory is now, in 2010, the principle workhouse and standard authority for the calculation of astronomical ephemerides.

5.6 Numerical Integration of the Outer Planets

The origins and details of the SSEC’s numerical integration of the outer planets are less definite than those of the lunar ephemeris. As with the initial calculation of lunar position, achieving greater accuracy was one goal. Eckert, director of the Watson lab, Clemence, the director of the NAO and Brouwer, director of the Yale Observatory, worked together to plan and complete the computation. The project was one of the first in a long term contract from the Office of Naval Research (ONR) that began in 1947. The U.S. Naval Observatory, Yale and the Watson Lab undertook the contract to explore the potential of new computing machinery for improving the accuracy of planetary theory and would shape research at the Naval Observatory for more than a decade. In addition to the numerical integration of the outer planets the ONR’s contract also mandated improved measurement of the satellites of Saturn, refinement of the theories of Mars, Jupiter’s satellites and the first four asteroids (by order of discovery). The results of the numerical integration became the basis for the British and American Ephemerides of the outer planets in 1960 and continued to be the basis of those tables until 1983. The immediate publication of the work as *Coordinates of the Five Outer Planets 1653-2060* in 1951 as volume XII of the irregular (and long running) series *Astronomical Papers*
Prepared for the Use of the American Ephemeris and Nautical Almanac\textsuperscript{8} suggests its importance. (Dick 2002, 526-527, 541-542; Wilkins 1961, 15-18, 111-112)

As with the lunar theory, Eckert and his colleagues planned the numerical integration before the SSEC was completed. Unlike the lunar work they planned the full scope of the work from the start. They completed the computation in 1950 and remark elsewhere that the work took 3 years to prepare. In addition, the organization of the observations used to check and refine the calculations, were themselves the work of several years and thousands of hours of expert analysis. Much of this work was apparently done before even the planning for the SSEC calculation began. (Clemence \textit{et al.} 1960, 93, 99; Eckert \textit{et al.} 1951, v-vi)

As previously mentioned, numerical integrations had been used before in cases such as Halley’s comet and by Eckert and others in the orbits of asteroids. Numerical integration had also been used to introduce corrections to orbits calculated by algebraic approximations. However, before the SSEC computation, almost no long term orbit for a major planet had been calculated in this way and none had been published. Another first in this computation was that this numerical integration treated the interactions of all five outer planets and the sun. In previous integrations, such as those performed by Eckert in the 1930s, only the trajectory of the body of interest was subject to integration; the other motions were simply given a preordained path. This did not affect the accuracy because the bodies in question did not sensibly perturb the motion of the much larger bodies, such as the large outer planets. However the outer planets integrated in this numerical integration are the largest bodies in the solar system, excepting the Sun, and their motions are significantly interdependent. So the mutual interactions of each body on the other, including perturbations of the Sun’s motion, had to be calculated to achieve greater accuracy. As a result the integration of the outer planets was a much more complex undertaking than previous numerical integrations. (Clemence \textit{et al.} 1960,

\textsuperscript{8}The monograph was only volume 12 in a series beginning with the work of Newcomb in 1882.
This integration of the outer planets represents the first time numerical integration was used, not for a special case like Halley’s comet, but for several major bodies in the solar system in combination over a considerable epoch. A little less than 20 years later, Schlomo Sternberg would note the rise of what he called “brute computation,” numerical integration by computers in celestial mechanics, and commented: “In a very real sense, one of the most exalted of human endeavors, going back to the priests of Babylon and before, has been taken over by machine.” (Sternberg 1969, xvi) Given the epic drudgery of celestial mechanics before the advent of automatic calculation, this lament seems ill-motivated. Despite the power of computers for such numerical work, Eckert and others would continue pursuing other “subtler” approaches as will be seen in the next chapter. Finally, Sternberg’s comments give short shrift to the amount of human input required even for numerical integration, the particulars of which we will turn to now.

5.7 Computing the Coordinates of the Outer Planets

As with Eckert’s previous method of numerical integration, once initial values had been determined and calculated there are two stages to the calculation iterated over and over. First the accelerations on the planets at time \( t \) must be calculated. Since motion was calculated relative to the position of the sun, any acceleration of the sun was represented as an acceleration of the various outer planets. The acceleration for a given planet results from the direct effect of the Sun and other planets on the planet and the indirect effect of the acceleration of the Sun by all the planets. The acceleration due to the inner planets was approximated by adding their collective mass to that of the Sun for calculation of the solar attraction. The masses of the planets were given as fractions of the sun mass or, to put it another way, the mass of the sun was set to 1. The second part of the calculation consisted in extrapolating from the current positions at time \( t \) to some new
set of positions at time $t + \Delta t$. The positions were calculated at 40 day intervals at noon for the Greenwich meridian and according to the Julian Date, as used by astronomers.\(^9\) (Eckert et al. 1951, ix-xi)

The Newtonian formula for the gravitational force ($F$) exerted by one body (mass $m_1$) by another (mass $m_2$) some distance ($d$) apart is given by the relation:

$$F = G \frac{m_1 m_2}{d^2}$$

where $G$ is the gravitational constant. The acceleration of a body in a given direction is then found simply by dividing out the mass of the body. The component of acceleration ($a_1$) in a given direction for a body $m_1$ would then be given by the formula:

$$a_1 = G \frac{m_2 x_{12}}{d^3}$$

where $x_{12}$ is the distance between the two bodies in the x direction.

For a given planet there were three sets of terms. The solar attractions were the acceleration due to the sun (mass of 1) and inner planets (mass of $m_0$) and the acceleration of the sun due to the planet (mass of $m_i$) in question, the direct perturbations were the accelerations due to the other outer planets (mass of $m_j$) and the indirect perturbations were the accelerations of the sun due to the other outer planets.

The solar attractions ($S$) for a planet were given by equations of the form:

$$S_{xi} = -G \left(1 + m_0 + m_i\right) \frac{x_i}{r_i^3}$$

where $x_i$ is the x coordinate of the planet and $r_i$ the distance between the planet and the sun (the origin of the coordinate system). The direct perturbations ($T$) on one planet due to another planet, in a given direction, was given by equations of the form:

$$T_{xij} = G \frac{m_j (x_j - x_i)}{\Delta_{ij}^3}$$

\(^9\)The Julian Date is defined as the number of mean solar days since noon January 1, 4713 B.C.E. in the Julian proleptic Calendar. For example, noon December 31st (January 0 in astronomical reckoning), 1899 is Julian Date 2 415 020.0. (Wilkins 1961, 71)
where $\Delta_{ij}$ is the distance between planets $i$ and $j$. Finally the indirect perturbation ($U$) of one planet by another (planet $j$), or the motion of the sun due to the other planet, was given by equations of the form:

$$U_{xj} = -G \frac{m_j x_j}{r_i^3}$$

Taken all together with Jupiter, Saturn, Uranus, Neptune and Pluto designated by the numbers 5-9 respectively, the expression for the planetary equations were as follows:

$$X_i = S_{xi} + \sum_{j=5, j \neq i}^{9} (T_{xij} + U_{xj})$$

where $X_i$ is the acceleration of planet $i$ in the $x$ direction. There were two analogous equations for the acceleration in the $y$ and $z$ directions ($Y$ and $Z$). Each time new positions were generated these acceleration terms had to be generated. (Eckert et al. 1951, ix-x)

The actual process of generating new coordinates at the next time was a further modification of Cowell’s method (the method Eckert used in the 1930s previously described). Instead of actually computing a full set of differences (and summations), the integration from one time to the next was achieved by use of previous values of the acceleration. The derivation apparently proceeds on the assumption that the tenth differences of the acceleration were zero. This was an approximation, since they will be non-zero but (hopefully) small. The expressions for the integration had the form:

$$x(1) = "X(1) + A_0 X(0) + A_1 X(-1) + A_2 X(-2)\ldots + A_9 X(-9),$$

where $A_n$ are constants with values between -13 and 16 whose sum is 1/12. Again there are analogous expressions for the $y$ and $z$ directions. Also, much of the integration involved an integration backward in time and so used this formula with the arguments suitably rearranged (multiplied by negative one). The second summations, "$X(0)$, was derived from the following expression:

$$"X(1) = 2"X(0) -" X(-1) + X(0)$$
rather than by carrying a long running tabulation of summations or differences. At the end of the computation the second summations were differenced to their twelfth differences (tenth difference of acceleration) and checked for indications of error (large differences). (Eckert et al. 1951, x, xi, xiv)

So once begun, the integration was a fairly straightforward process. However, as indicated by the equations, an initial set of positions, ten for each of the 5 planets, had to be determined and the associated accelerations (and summation terms) calculated. The choice of initial values was crucial to the entire rest of the computation. These values were first derived from existing ephemeris tables. However such tables were not necessarily well suited to the problem of giving such initial conditions. In order to check their accuracy, extensive trial integrations were carried out and then compared with previous theory and observation to obtain corrections for the starting values. Three such test integrations were done: one for the period 1910-1941 and two for 1780-1960. The start dates selected for the integration are centred around January 6, 1941, this is the so-called osculatory period where the forward and backward integrations meet. The forward and backward integrations were started at different times creating an overlap for purposes of comparison. Watson Laboratory staff member Eleanor Krawitz (c.1925-) assisted in preparing the data for the integration and in the numerical analysis that followed. (Eckert et al. 1951. vi, xvi-xvii)

After each test integration, Eckert and his colleagues used differential correction to find better initial coordinates. This procedure resembles other curve fitting procedures such as least squares analysis in that equations of condition are found to fit the integration to existing values for the position derived from previous theory or observations. In this case the equations of condition give a formula for generating corrections to the coordinates given by the integration, correcting it to be in-line with the benchmark values. These corrected coordinates can then be used to refine the initial conditions used
in the numerical integration. The equations of condition have the form:

\[ \delta x = a_1 \xi_1 + a_2 \xi_2 + a_3 \xi_3 + a_4 \xi_4 + a_5 \xi_5 + a_6 \xi_6 \]

where \( \delta x \) is the difference between the integration and the benchmark values for planetary position, \( a_n \) are constant coefficients and \( \xi_n \) are variables dependent on the positions generated by the numerical integration and also on the velocities of those positions. There are similar equations of condition for the \( y \) and \( z \), except that the coefficients \( a_n \) are replaced by \( b_n \) and \( c_n \) respectively. A minimum of six differences between the integration and the benchmark positions would be required to solve the equations of position. In discussion of the results, Eckert and his colleagues suggest nine differences as a representative example of the number of data points used to calculate the coefficients for the corrections. The points of comparison were apparently calculated at constant intervals of 1000 days for Jupiter, 1920 days for Saturn and 2000 days for Uranus and Neptune. (Eckert et al. 1951. xvi-xxi)

The first test integration over 30 years, 1941-1911, saw a straightforward application of the differential correction. The duration of this first test integration was chosen to match the period of Saturn’s orbit. The values obtained by this first integration were fitted against previous predictions. Jupiter and Saturn were compared against the tables of Hill that had served as the standard source of ephemeris values since 1895. The benchmark positions of Pluto were taken from Bower’s elements. The benchmarks for Uranus and Neptune were an older set of numerical integrations performed at the Watson Computing Bureau around 1940 as part of a project at Yale. The researchers focused their attention on corrections to the positions of Jupiter and Saturn, because Hill’s tables do not produce velocities, required for fitting the initial conditions, and differential corrections, with the same accuracy as the other benchmarks. Also, due to the inaccuracy of Hill’s tables for the radial component of the planets orbits, the corrections were based on the contributions only from the longitude and latitude. Eckert and his colleagues took special care to obtain the maximum possible accuracy from Hill’s table and carefully chose the points
at which comparison would be made. Hill’s values were also compared against a previous computation of Jupiter’s position done at Yale. Saturn’s positions were checked by means of a special differencing technique. (Eckert et al. 1951, xviii-xix)

On the basis of the corrections derived from the first integration, they undertook a new integration for the dates from 1780 to 1941. Although no rationale is given, three reasons for integrating over this period suggest themselves. First, 1781 marks the discovery of Uranus, second, the period of Neptune’s orbit is 160 years and third, the period of the 1940 numerical integration of Neptune was for the period 1780-1938. The numerical integration actually proceeded backwards, starting with the data from 1938 and calculating previous positions. When they attempted to perform further corrections on the new integration based on the benchmark values, they found the differences for Saturn could not be accommodated by the usual process. A comparison with observations found that the test integration agreed more with the observations of Saturn than with the figures found via Hill’s table. Similarly the corrections for Jupiter were unexpectedly large. Therefore, they used the longitude generated by observations and Hill’s values for latitude to obtain the next set of corrections for Jupiter and Saturn. (Eckert et al. 1951, xix-xx; Brouwer 1940, 7)

At the same time the benchmarks for Uranus and Neptune were further refined by the addition of the perturbations of Pluto and corrections based on the observations. During the comparison between the test integration and the benchmark values of Neptune and Uranus, a discontinuity was discovered that revealed a mistake in the computation of the 1940 numerical integrations. The error consisted in a single sign error in the z coordinate for the perturbing planet Jupiter for a date in 1867. This error meant that the old numerical integration of Neptune and Uranus would be in error for the period before 1867\(^{10}\), because of the erroneous perturbation. The error had meant that it was

\(^{10}\)The period before 1867 was affected because the numerical worked backwards in time from 1938 to 1867 and earlier dates.
not possible to reconcile observations of Uranus and the old integration before 1830, therefore such observations were not incorporated into the improved benchmark. Since the period of significant deviation was before the discovery of Neptune, the mistake was insignificant for the benchmark values used for that body. (Eckert et al. 1951, xx-xxi)

Based on the corrections to the planets’ positions they generated a new set of values for the final test integration, also for the dates 1780-1960. Comparing the new sets of planetary positions to the benchmark revealed that the new test integration fit with the benchmarks to within the margin of probable error requiring no corrections. Therefore, this integration became the core of the final integration forward to 2060 and back to 1653. (Eckert et al. 1951, xix-xx)

The main product of the numerical integration was the tables of planetary positions published as the Coordinates of the Outer Planets. These tables were in most cases significantly better fits to the observed values than the previous standards used by the Anglo-American ephemerides. Two sets of graphs suggest the improvement achieved by the numerical integration. Figure 5.4 shows the deviation from observations of the old tables and figure 5.5 shows the deviation found with the new numerical integration, both for the period 1780-1940. Note that the values for Uranus and Neptune are for the tables of Newcomb of 1898 and not the 1940 numerical integration used to fit the parameters of the numerical integration. Also, the old tables for Jupiter and Saturn had been subjected to a correction of the orbits and represent a smooth averaging of residuals. These graphs show that the improvement in agreement with position for Neptune was the greatest, reducing both the maximum error and the average error. Saturn and Uranus saw improvement more distinctly in terms of average error with little improvement in the maximum error. Any improvement for Jupiter was not apparent. (Clemence et al. 1960, 97-100; Wilkins 1961, 191, 197)

As with the discussion of the lunar ephemeris, a great deal of discussion of the results for the Coordinates deals with issues of the precision of the numerical integration as
Graph of the difference between positions predicted by old tables and observations.

Figure 5.4

(Clemence et al. 1960, 98)
Graph of the difference between positions predicted by SSEC numerical integration and observations.

Figure 5.5

(Clemence et al. 1960, 99)
opposed to its empirical accuracy (the graphs in Figures 5.4 and 5.5 were part of a separate discussion). The acknowledged sources of error in the calculation included treating differences higher than the 10\textsuperscript{th} difference as equal to zero and rounding errors. Throughout the calculations 14 significant digits was the limit of precision on multiplication and division. Since the accelerations and other terms due to various planets varied by orders of magnitude, the precision for some terms was less than 14 digits. The final results were printed to nine digits of precision, but all the positions and accelerations were also stored to full precision in punch card form. The initial error analysis suggested that the error from all sources would be at most a single unit in the ninth decimal (±1). As mentioned previously the resulting coordinates were tested by finding their tenth differences and ensuring they remained small. Also, again as in the lunar problem, each individual calculation was run in parallel, each recorded separately with numbers multiplied by 0.99 for the check calculation and checked at fixed intervals. If a discrepancy was detected by the comparison the arithmetic was rerun. If the discrepancy was not resolved by the recalculation then the machine stopped. As with much of Eckert’s output, the tabular data was first organized and checked in punch card form. (Eckert \textit{et al.} 1951, XIII-XV; Clemence \textit{et al.} 1960, 97)

In addition to the discussion in the original publication of the \textit{Coordinates of the Outer Planets} subsequent analysis of errors and the calculations of corrections occurred. Clemence carried out a set of corrections taking into account the perturbations caused by the inner planets. These values had not been included in the original \textit{Coordinates} because the authors deemed them “scarcely perceptible.” The corrections would be used to modify some, but not all, the coordinates given in the British and American national ephemerides based on the numerical integration work. (Eckert \textit{et al.} 1951, V; Wilkins 1961, 111)

In a separate endeavour, Clemence and Brouwer carried out a series of calculations to show the integration was consistent with the fundamental laws of motion. The check
consisted in finding integrals at various time steps of the integration: one integral of the total energy and three integrals, referred to as integrals of area, for the angular momentum. In principle these integrals should remain constant for all times. These calculations were achieved with the aid of the full precision values of the acceleration and associated first and second summations kept at the Watson Laboratory. Clemence and Brouwer were assisted in planning and executing their numerical work by lab researcher John Lentz. They found that most of the deviations in the integrals were in or near the ranges expected, but one of the integrals of area had deviations six times larger than expected. Even with this large unexpected variance, the precision of the calculation exceeded the nine digits printed. However, they also pointed out that the precision of the calculations was far in excess of the expected accuracy, because of uncertainties in the masses of the planets. (Clemence and Brouwer 1955, 118, 123-126)

An important secondary result of the numerical integration was to help refine estimates of the mass of Pluto. Part of the impetus for the optical search that led to the discovery of Pluto was unexplained deviations in the motion of Neptune and Uranus. Attempts to estimate the mass of Pluto depended on the perturbations of Neptune that could be attributed to Pluto, with some corroboration from optical measurements of Pluto’s size and apparent luminosity. The mass of Pluto used in the SSEC integration (1/360 000 of a solar masses) had been derived by calculations based on the residuals of Neptune. It is unclear what the exact source of the estimate used in the integration was, except that the source of the estimates of the unexplained residuals was the work of L. R. Wylie, published in 1942. Wylie’s own calculations using these residuals found a slightly larger mass (1/330 000 of a solar mass or 1 Earth mass). Brouwer had worked with Wylie on that estimate and suggested a reduction in the mass estimate. Brouwer had been engaged in a project to calculate the mass of Pluto based on the residuals of Neptune and Uranus using punched card machines and his preliminary result in 1940 had suggested a lower mass for Pluto than either Wylie found or than was used in the inte-
Chapter 5. Eckert’s Work on the SSEC

5.8 Features of the SSEC Displayed in the Outer Planets Integration

The prospect of using the SSEC for numerical integration had been in Eckert’s mind since the early stages of the design process. In his initial specifications for the SSEC he noted...
how successful his use of earlier machines for planetary orbits had been. He suggested
the reuse of the routines carried out on the old machines for SSEC. Although this specific
suggestion was not taken up, the SSEC proved capable of carrying out a far more complex
integration than he first conceived. As with the calculation of lunar positions the machine
programming and operation were handled by a team of technicians. In this case they
were William McClelland, Phyllis Brown and Womble. (Eckert MS 1945, 8; Eckert et
al. 1951, VI) Unfortunately Eckert never completed the second edition of Punched Card
Methods in Scientific Computation, which was intended to describe, among other things,
the numerical integration of the outer planets. However published reports and archival
material give some sense of the machine features involved in the calculation.

As mentioned in the previous chapter the tape punches of the SSEC never worked
as intended. However with some modification they were able to store and read values
generated during the course of the SSEC’s operations. According to the reminiscences of
the SSEC’s supervising engineer, A. Wayne Brooke, the numerical integration problem
was the only problem to make use of the large reserve of storage capacity in the tape
punch. An example of the need for storage was that 150 numbers calculated in previous
steps of the integration must be referred to at each new step. The calculation of a
new coordinate depended on ten previous values of the component acceleration with
five planets each having three coordinates and associated component accelerations. This
requirement is doubled by the use of the parallel computation as a check because all
the instructions and numerical factors used were independent for each computation.
Therefore the 150 numbers of relay storage would be outstripped. The placement of
10 punch readers on each of three paper tapes meant that if the punches and readers
were optimally spaced the previous accelerations required for the calculation could have
been read off the tapes without spending time searching the tape. (Brooke MS no date,
42; appendix B MS no date, pp. B8; Eckert et al. 1951, XI, XIII)

In fact the over 3500 steps of the integration (and the parallel check calculation)
would have required storing on tape perhaps 30 numbers at each step, outstripping the 20,000 numbers of maximum possible storage on the tapes. Presumably the operators performed the integration in separate runs of the machine, replacing the large tape rolls. The only other recorded use of the punched tape I have found was to store a short set of intermediate values that were then cut out and pasted in a closed loop for reading into the next stage of the problem. (Appendix B MS no date, pp. B9-10) Therefore having to replace the tapes over the course of computation may have been a standard step in SSEC calculation.

Despite the limitations of the paper tape punches and readers, their use in the outer planets integration demonstrated the value of the high capacity of the SSEC for number storage. This feature of the SSEC had been envisioned during the design phase of the machine’s construction and Eckert had set the large capacity in his initial specifications. Since Eckert’s specifications represented the results of several previous meetings and discussions, the emphasis on large storage cannot be attributed solely to him. However, his extensive experience with scientific computation may have suggested the need for the large recording and storage facilities. These did have value for scientific work. The ENIAC was considered less suitable for problems such as hydrodynamic flow work because of its smaller storage capacity and slower input and output facilities, despite being faster in terms of fundamental arithmetic than the SSEC. (Eckert MS 1945, 2; Bashe et al. 1986, 57; von Neumann 1961, 666)

Unlike earlier numerical integrations performed by Eckert this method did not use differences explicitly in the process of integration. Instead the process used in integration extrapolated by summing 10 past values of the acceleration multiplied by constants and the second summation of the acceleration. However, the process is equivalent to the old method taken out to the tenth difference. The reason for the change is not made explicit, but a reasonable inference is that the change was made to avoid having to store or constantly recalculate the multitude of differences that would be required. The
integration step of the old method only required the even differences and so would have only summed 7 terms instead of the 11 used in the actual computation and would similarly have avoided 4 multiplications at each step.\textsuperscript{11} However to calculate a difference to the tenth order requires two ninth differences, three eighth differences and so on, with the tenth difference and initial accelerations (10) this amounts to 55 numbers in total for the initial step for each part of the acceleration of each planet. It also requires a set of associated subtractions to calculate the new differences. Although only 10 numbers per term would be required to generate subsequent 10\textsuperscript{th} differences, ten intermediate even differences used in the formula would have to be stored at all times also.

The advantage of the difference method on the older punch card machines was the speed of addition or subtraction versus multiplication, the simple steps involved and the automatic check on the calculations provided by the differences. The time required for multiplication on the older machines was ten times that required for addition or subtraction. While, the basic operations of addition and subtraction were even faster relative to multiplying speed on the SSEC, the time taken to manipulate numbers in storage was much larger relative to multiplying time. In the multipliers of the 1930s, multiplication speed was the bottleneck. In the SSEC accessing relay memory took as long as a multiplication and accessing the paper tape took more time than a multiplication. As mentioned previously, the differences were calculated to check for errors in a separate computation. In fact they performed the differencing on a separate machine, an IBM 602-A Calculating Punch, the last electromechanical machine in IBM’s multiplier line.\cite{Eckert1951, Eckert1940a} Again this avoided wasting valuable time on the SSEC on a

\textsuperscript{11}For comparison the equations used for the SSEC have the form:

\[ x(1) = x''(1) + A_0 X(0) + A_1 X(-1) + A_2 X(-2) ... + A_9 X(-9) \]

\cite{Eckert1951, Eckert1940a} While the 1930s machines used the following equation:

\[ x(1) = x''(1) + C_0 X(1) - C_1 X''(1) + C_2 X^{IV}(1) + ... + C_5 X^X \]

\cite{Eckert1940a, 101-104}
very simple set of operations.

The IBM 602-A was also used for some other bookkeeping. As mentioned previously, the integration measured time in the Julian day format, but the resulting tables needed to be given in the modern Gregorian calendar (Universal Time). The calculation and printing of the corresponding calendar dates was performed in one run on the IBM 602-A. Electromechanical and controlled by plugboard, the IBM 602-A was more complicated than the IBM machines of the 1930s and could perform multiple operations, up to 12, on a single card. (Eckert 1951, 13; IBM 1958, 5-6)

As with much of Eckert’s work, the tables for the *Coordinates of the Five Outer Planets* were prepared directly from punched card output. Eleanor Krawitz checked and arranged the tables and Rebecca Jones prepared the copy of the tabular matter for direct reproduction by the printer. Presumably, as with Eckert’s NAO projects during the war, the tables were proofread by punching the numbers in the printed proof and comparing them with the punched output of the SSEC. In addition to the check on the raw numbers, the rounded values of the coordinates were themselves differenced to the eighth difference to insure that rounding had introduced no errors. (Eckert *et al.* 1951, VI, XV)

The switch away from mathematical tables with the advent of the SSEC can be again seen in the use of an iterative algorithm to find square roots in this problem. The distance (from the Sun or from given planets) is given at each step in the integration only indirectly from the component coordinates of position (x, y and z). The equations of the integration require the cube of the distance ($r^3$) as a divisor in addition to the components coordinates. Therefore it was necessary to calculate those distance values. The sum of the squares of the components is the square of the total distance $r^2$. The natural way to calculate $r^3$ is thus to first calculate $r^2$ find its square root and then cube that value. By contrast, in the previous punched card integration technique, rather then directly find the value of $r$ by taking the square root of $r^2$ and then calculate $r^3$ for use in division, a table in punched card form indexing the values of $1/r^3$ to the values of $r^2$
was used to derive the necessary values. (Eckert 1940a, 107-109) The direct method was adopted on the SSEC for the numerical integration problem.

In the SSEC calculation the distance was found from its square by iteration of the formula:

\[ q_{i+1} = \frac{1}{2}(q_i + N/q_i) \]

where \( q \) is the approximation to the square root and \( N \) is the number whose root is being taken. The formula was run through a minimum of 4 iterations and repeated as necessary to achieve required precision. In this way \( r \) was derived and then it was a straightforward matter to cube the number and use it calculations. (Eckert et al. 1951, XII) This situation again suggested that in the SSEC, data handling had become comparatively more expensive than multiplication and led to a move away from tables.

One other machine feature that dictated a feature of the computation was its obligatory use of fixed point arithmetic. In fixed point some digits are significant numerical factors while others may exist simply to indicate the proper magnitude in arithmetic. Therefore a computation might need to make a trade off between significant digits and maintaining the proper magnitude. By contrast the floating point representation keeps track of the magnitude and actual digits separately. In preparing for the calculation of the acceleration the \( x, y \) and \( z \) coordinates were multiplied by various scaling factors (depending on the planet) in order to insure calculated values of the distances (\( r \) and its cube \( r^3 \)) to the sums of those values would be slightly less than \( 10^n \). The distances between the planets (\( \Delta \)) would be multiplied by either 0.1 or 0.01 depending on the size of \( \Delta^2 \). The scaling factors were then removed with the completion of the calculation of acceleration. (Eckert et al. 1951, XII-XIII)

Presumably the factors applied to \( r \) were meant to combat the loss of precision experienced by a number slightly larger than 1 compared to one just below one.\(^{12}\) While, the

\(^{12}\)Take for example a case where only 3 digits can be kept and adding 0.002 to 0.999 obtaining 1.00 versus scaling by a factor of 0.998 and adding 0.001996 to 0.997002 and obtaining 0.999 (implicitly over 0.998). The imprecision in the first case is much larger than in the second.
modification of $\Delta$ presumably eliminated the contribution from the rightmost digits of the numbers. This would presumably avoid the numbers overflowing the capacity of the computer storage on the left digits. Also, the eliminated digits would have very small absolute values and therefore would not contribute greatly to the precision. The scaling factors for $r$ might make a difference even in floating point, but those for $\Delta$ (which merely altered the magnitude of the terms) would not have. The use of the scaling factors also suggests the lengths that were taken to obtain the best use of the SSEC in these calculations.

5.9 Impact of the SSEC work: End of the Beginning

The details of the calculations performed on the SSEC and their subsequent analysis suggests the continued need for careful human analysis of the results of computer calculation. They suggest a continued vigilance by the investigators in avoiding mistakes either mechanical or theoretical. However, the extensive error checking done on the SSEC problems did not represent a distrust of the machine’s reliability or unease with a new way of doing things. On the contrary, Clemence, Brouwer and Eckert speculated that the Numerical Integration of the Outer Planets might have been impossible without the superior reliability of the SSEC’s arithmetic as compared to human efforts. (Clemence et al. 1960, 97) Rather, the extensive checks against previous theories represented the aspirations for precision and accuracy demanded of new theories in celestial mechanics. In this case the new theories Eckert and his colleagues devised with the aid of the SSEC pushed the accuracy of theory beyond that of the limits of observation. The emphasis on comparison with previous theory also shows how older theory was viewed by astronomers as an embodiment of the past data to which they were are a good fit.

The problems Eckert worked on with the SSEC consisted of apparently pedestrian processes of prediction that lacked any obvious novelty. However, Eckert saw his work
as an extension of the centuries old project of extending and testing the limits of the theory of gravity. As he stated it: “[T]he classic problem [computing of celestial motions] remains one of our principal avenues toward a better understanding of the fundamental nature of things.” (Eckert 1957, 43) For the most part, the tables of the Moon and outer planets developed on the SSEC served to confirm that existing theories of gravity and motion were adequate. However as seen in the calculation of the mass of Pluto, the increased accuracy of the SSEC work held out the hope for the refinement of the physical parameters of the solar system. The increased accuracy of the Improved Lunar Ephemeris was also seen as a way to improve the measurement of time in astronomy and so potentially was of a broader application.

Eckert, like other computer pioneers, saw the electronic computer as an extension of the scientists power’s, akin to the microscope or telescope but in this case extending the reasoning powers. For Eckert this analogy was deep, since he argued that more computation could be used as a substitute for more observations, more accurate observations or for qualitative mathematical theory. (Akerja 2007, 127; Eckert 1948b, 320)

In its most obvious achievement, the SSEC shrunk the time required to develop a new theory of celestial bodies. Previously the development of more accurate lunar and planetary tables had required a decade or more of calculation. The SSEC managed to produce new and more accurate tables for the positions of the Moon and the five outer planets in just a few short years. A striking aspect of these new theories was the rise in scale and scope of the numerical integrations performed on the SSEC compared to previous efforts, though this was not its only implication.

The use of the SSEC also brought about subtler changes to the way astronomical work was carried out. The example of the change away from trigonometric tables for lunar position calculations and the use of scaling factors to help optimize the accuracy of calculation suggest the way problems had to be tailored to the SSEC. The multiple staff required to program the SSEC also suggests how much effort went into arranging the
problem for the machine. The SSEC’s transitional status showed itself in the shifting mix of techniques it used and the way Eckert and his team made use of its various features.

The switch to automatic machine calculation also had indirect effects on astronomical work. The ability to store intermediate values and other unpublished aspects of work in machine form allowed a greater ease in performing subsequent analysis on the work, at least in the short term. Eckert’s projects were large collaborative efforts as opposed to the smaller staff size of efforts like that of his supervisor E. W. Brown or even his earlier work in the 1930s. Partly these larger teams represent his involvement with a network of large organizations in Yale, the NAO and IBM. However, part of the source of interest, resources and money for these projects was the exploration of the uses and limits of the new electronic calculating technology.

While describing the potential applications of the SSEC Eckert made the following point. “It is possible to state these laws [Newton’s laws of motion and gravitation] in a few sentences, but their application to the various crucial cases has required the greatest efforts of mathematicians and astronomers for a century and a half, and the work already done is just a beginning.” (Eckert 1948b, 320-321) Similarly Eckert’s work on the SSEC heralds the beginning of a new chapter in celestial mechanics, the use of computers to solve the problems of celestial mechanics.
Chapter 6

Eckert’s Later Work on Lunar Theory

As the field of computers developed from experimental machines to production line models, Eckert’s involvement with the cutting edge of computing technology lessened. Eckert ceased to be a designer and became a user of the computer as it matured. He remained the director of the Watson lab at Columbia until 1966, but after 1957 his published output focused almost exclusively on lunar theory. He would continue to work on lunar theory until his death in 1971. It was for these contributions to lunar theory that he would be best remembered by his colleagues in astronomy.

6.1 Eckert’s Continued Work on Airy’s Method for Lunar Theory

As discussed in chapter 3 Eckert, with encouragement and help from E. W. Brown, had used punched card machines to carry out the determination of corrections to Brown’s lunar theory. The method used was first devised by George Biddell Airy, discussed in Chapter 2. The results were never published in full, however they confirmed that the
precision of Brown’s theory, the equations agreement with Newton’s laws, was within Eckert and Brown’s expectations. A manuscript in Eckert’s papers detailing the method and setting out the results, with corrections dated from the fall of 1943, indicates that Eckert had undertaken much of the work required to publish the results. Other marginal notes that post-date the 1943 revisions suggest that, when he returned to New York at the Watson Computing Laboratory, he once again took up the task of publishing the results. Yet the results of the 1930s investigation were never published. (Eckert MS 1943, 2, 6, 18, 20, p. 2 verso)

The reason for the lack of publication is unclear. Judging from what discussion of the work survives, it was never carried past calculating the residuals to determine a new corrected trajectory for the Moon using equations of variation (curve of best fit). A cryptic handwritten note found on the manuscript suggests one possible explanation. The note reads: “Parallax in error by .001. Periodic terms no mult by $1 - \frac{L}{300000}$ on insignificant terms in some graphs. ∴ [Therefore] results not reproducible” (Eckert MS 1943, 3A verso) This suggests the residuals derived may have been in error or simply failed to offer the hope of a significantly more precise theory. On the other hand, one of the notes associated with the manuscript is dated 1958 by which time a new effort to calculate a more accurate lunar theory using Airy’s method had been undertaken. (Eckert MS 1943, 53) It may simply suggest the results of comparison of the latest calculations with the earlier efforts.

Whatever the reason for the non-publication of the 1930s work, Eckert resumed his project to calculate lunar theory by the method of Airy in 1957. The project began by recalculating the residuals for the same parameters and precision as the 1930s calculation as a check on the consistency of the method. However, with new high speed computers readily available, Eckert planned to go beyond the original project once the original results were reproduced. The new goal was not merely to correct for the large error in the motion of the nodes and perigee, but also to increase general accuracy of the theory
of the Moon. Another consideration was the increased accuracy of the parameters now in use. He was assisted in the creation and design of the plan and programs for calculation by Rebecca Jones. The machine work began in May with one Miss Lydia Stenbo punching out the values of the coefficients of all the terms from Brown’s theory. By June of 1957 Eckert had laid out a set of calculation procedures that were checked by Jones. The initial calculations were executed on the IBM 650 computer with some of the initial card preparation and calculation done using an IBM 604 accounting machine. (Jones MS 1957, 1-2).

This initial work proceeded quickly and by March of the following year the initial check against the 1930s values had been completed and the precision extended. The planned extension of the work meant taking into account smaller terms in the calculations of the residuals. The initial public presentation of the work occurred in March of 1958 at a celestial mechanics conference. This conference had been conceived in the summer of 1957, but its program was recast in light of the launch of Sputnik 1, the first artificial satellite, by the Russians in the fall of 1957. Some of the participants at the conference addressed these recent developments directly with papers on orbit determination for the artificial satellites. However, the space age also had implications for the more old fashioned celestial mechanics of Eckert. Artificial satellites were one among many new sources of data projected to go on line in the near future. Since artificial satellites could be given arbitrary trajectories around the Earth, they offered the potential for measuring various gravitationally significant features of the Earth such as its exact shape. Eckert himself stated that new methods of observation were a consideration in the work to extend Brown’s theories. (Eckert 1958, 415-418)

Eckert and Jones’s initial work with the IBM 650 involved the calculation of the residuals of Brown’s theory. As explained in Chapter 3 most of the work of obtaining the residuals consisted in calculating the products of long series of trigonometric terms, multiplying the coefficients and then transforming the product of the two trig functions
into the sum of two different trigonometric functions. Some of these terms recurred frequently while others would not. In light of this, Jones and Eckert found it useful to store the recurring terms in the high speed table-look-up provided by the 650. Around 1500 of 30 000 terms would be stored in the table-look-up when multiplying a given series. (Eckert 1958, 416-417)

In 1958 Rebecca Jones left the Watson Laboratory and ceased assisting Eckert in any of his projects. It seems that at some point soon after, Harry F. Smith Jr. came on board the project. Smith was already a worker at the Watson lab, having started in 1956 while still an undergraduate at Columbia. He received his BSc from Columbia University in 1957, and completed a Masters in Applied Mathematics in 1960. Smith began assisting Eckert with the understanding that the work would be the basis of his doctoral thesis. (Brennan 1971, 61; Smith MS 2007; Smith MS no date)

Having carried out the basic computation of the residuals, they began to implement a method to find the corrected trajectories. The complete solution of this problem, to its ultimate precision, took well over a decade of computation and planning. The machine computation started on the relatively small, slow and old IBM 650 and would extend to larger, faster and more advanced transistor machines.

As discussed in chapters 2 and 3, Airy’s method, as employed by Eckert and Smith, rests on three differential equations into which they substituted a trial solution. In subsequent discussions the forms of the equations used were slightly modified and a fourth equation added to give a residual for certain constant terms. These equations are
as follows:

\[
\begin{align*}
\frac{d}{dt}(x y - y x) + (x^2 + y^2) &= \int \left( \frac{x \delta \Omega}{\delta y} - \frac{y \delta \Omega}{\delta x} \right) dt + C_1 \\
\frac{1}{2} \frac{d^2(r^2)}{dt^2} + 1 \frac{2(x^2 + y^2)}{2} - \frac{1}{2} \left\{ \left( \frac{dx}{dt} \right)^2 + \left( \frac{dy}{dt} \right)^2 + \left( \frac{dz}{dt} \right)^2 \right\} &= \\
\Omega + \left[ r \frac{\delta \Omega}{\delta r} \right] - \int \frac{1}{n} \frac{\delta \Omega}{\delta t} dt + 2 \int \left( \frac{x \delta \Omega}{\delta y} - \frac{y \delta \Omega}{\delta x} \right) dt + C_2 \\
\frac{1}{2} \frac{d^2(r^2)}{dt^2} + 2 \frac{y \frac{dx}{dt} - 2 x \frac{dy}{dt} - x^2 - y^2 - \left[ \left( \frac{dx}{dt} \right)^2 + \left( \frac{dy}{dt} \right)^2 + \left( \frac{dz}{dt} \right)^2 \right]}{x \frac{d^2 z}{dt^2} + z \left( \frac{d^2 x}{dt^2} + 2 \frac{dy}{dt} + x \right)} &= \frac{\delta \Omega}{\delta z} - z \frac{\delta \Omega}{\delta x} \\
\frac{a a_0}{r} \approx \left[ \frac{\delta \Omega}{\delta r} \right]
\end{align*}
\]

\(x, y, z\) and \(r\) the coordinates of the Moon relative to the centre of the Earth with axis having the same motion as the average motion of the Moon, \(C_1\) and \(C_2\) are constants of integration. \(\Omega\) is the force function of the sun on the Earth and Moon. The basic relation to calculate \(\Omega\) is \(\Omega = \sum_j \sum_h k_{jh} \omega_{jh}\), where \(k_{jh}\) are constants and \(\omega\) is a function of \(r'\) to the power \(j\) (\(r'\) is the distance between the Earth and the Sun). Eckert, Jones and Smith calculated the four residuals as equal to the left hand side of each equation minus the right hand side. A perfect fit of the initial trial solution should lead to a residual of zero. The residuals were thus each a series and they designated those series as follows: \(\sum E_\xi \cos \zeta\) for equation (6.1), \(\sum F_\xi \cos \zeta\) for equation (6.2), \(\sum G_\xi \cos \zeta\) for equation (6.3) and \(H_0\) for equation (6.4). (Eckert and Smith 1976, 206-207, 222-223).

The trial solution (Brown’s theory) was of the form:

\[
x_1 = \sum x_\xi \cos \xi
\]

\[\xi = g_\xi F + c_\xi l + m_\xi l' + d_\xi D\]

\[\approx A_\xi nt + B_\xi\]

\[A_\xi = [g_\xi g + c_\xi c + m_\xi m + d_\xi (1 - m)]\]

\[B_\xi = [g_\xi (\epsilon - \theta) + c_\xi (\epsilon - \omega) + m_\xi (\epsilon' - \omega') + d_\xi (\epsilon' - \epsilon')]\]
where the coordinates are defined as in the differential equations above, and $x_1 = x - 1$. The other values are parameters specified by measurements or constants found in Brown’s solution. Of note are $x_{\xi}, y_{\xi}, z_{\xi}, c, g$ and $a_0$; these are the parameters modified to improve the solution. The quantities $c$ and $g$ are proportional to the motions of the Moon’s perigee and node and $a_0$ is a scaling factor. Also, $l, l’, F$ and $D$ are called the Delaunay variables.\(^1\) (Eckert and Smith 1976, 200, 206, 218, 222-223)

The corrections to be found in the solution according to Airy’s method were designated: $\delta x_{\xi}, \delta y_{\xi}, \delta z_{\xi}, \delta c, \delta g$ and $\delta a_0$. These symbols each represent a series of corrections, since there are corrections for potentially every term (with a different argument or period) in the series. The values of the corrections were determined by reference to the equations of variation and the residuals. Those equations were as follows:

$$\frac{\partial E_{\xi}}{\partial x_{\xi}} \delta x_{\xi} + \ldots + \frac{\partial E_{\xi}}{\partial a_0} \delta a_0 = \delta E_{\xi}$$

$$\ldots \ldots$$

$$\frac{\partial H_0}{\partial x_{\xi}} \delta x_{\xi} + \ldots + \frac{\partial H_0}{\partial a_0} \delta a_0 = \delta H_0$$

The solution of the equations was simplified in some cases as when there was a direct relationship between terms or where other simplifications are possible. However, when in 1961 Eckert and Smith reported on their progress so far, they commented: “The solution of the variation equation has proved more troublesome than we hoped.” (Eckert and Smith 1961, 447; Eckert and Smith 1976, 222-228) The variation equation would be the equation fitted to the residuals.

The corrections were determined by making various simplifying approximations, including neglecting the squares of the correction and many terms in $\Omega$. A single correction is required for the constant terms and given by the following expression:

$$3 \left( \frac{a}{a a_0} \right)^3 \frac{\delta a_0}{a_0} = H_0$$

\(^1\)The F in the Delaunay variables is a different variable from the F designating a set of residuals.
The corrections to x, y, z, c and g were numerous with one for each different argument of the trigonometric terms in the series. Some cases allowed greater simplification of the determination but most corrections for x, y and z were found based on the following relations:

\[
\delta x_\xi = \frac{2A_\xi x_\xi \delta A_\xi - F_\xi}{1 - A_\xi^2} \\
\delta y_\xi = -\frac{y_\xi \delta A_\xi + E_\xi + 2\delta x_\xi}{A_\xi} \\
\delta z_\xi = \frac{2A_\xi z_\xi \delta A_\xi - G_\xi}{1 - A_\xi^2}
\]

where \( \delta A_\xi = p_\xi \delta g + r_\xi \delta c \). The key feature of these relations were the denominators. When the denominator was near zero (i.e. when \( A_\xi \) approaches zero or 1) even slight uncertainty about the numerator would lead to a large range of possible values for the correction. This is the problem of small divisors that had long been an issue in celestial mechanics, as mentioned in Chapter 2, and it was this problem that apparently created the bulk of the difficulty for Eckert and Smith. (Eckert and Smith 1961, 447-448; Eckert and Smith 1976, 228-231)

The problem of small divisors had been an issue since the start of the project. E. W. Brown had suggested the use of a frame rotating with respect to the mean motion of the Moon, in part because it would more clearly reveal the error due to terms with small divisors in his original equations. In order to solve the problem presented in this case, Eckert and Smith turned to various methods in linear algebra and matrices. The solution of algebraic equations, by representing the relations in matrix form and solving by various matrix operations, predates the computer. However, the computer had proven an ideal method to represent matrices and perform matrix operations on them. Eckert and Smith’s attempt at the initial derivation of the residuals resulted in 3500 equations with an equal number of unknowns to be solved to determine the corrections. This would be expanded as the precision of the calculation increased and solutions were iteratively entered into the process to achieve greater precision, first to 6325 terms and then to over
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Effects of \( l \) and \( F \) variables on the magnitude of the corrections.

Figure 6.1

\[(Eckert \text{ and Smith 1966, 249)}\]

9000. (Brown 1938, 785-786; Eckert and Smith 1961, 447-448; Eckert and Smith 1976, 243, 248; Smith 1965, 30-44)

The organizing principle of the work relied on the Delaunay variables, since \( A_\xi \) depended on those values and in turn determined the large divisors. The variables \( F \) and \( l \) were particularly important. Both had values near 1, thus they could lead to sums of approximately \( \pm 1 \) or 0, for example an argument \( 2l-3F \) would give an argument near \( -1 \). This led them to a representation of these relations in terms of a schematic matrix with axis of \( F \) and \( l \) (see figure 6.1). The squares around the numbers indicate small divisors, the numbers without the squares interacted strongly with the small divisor terms. Terms with an argument containing even multiples of the variable \( D \) would have large coefficients making it strongly interacting. Also, note that even values of \( F \) indicate \( \delta x \) and \( \delta y \) terms and odd values indicate \( \delta z \) terms. (Eckert and Smith 1966, 248-249)

Two methods of solution were used by Eckert and Smith. The first was called the direct approach and involved inverting the matrix to find the values. The other indirect
technique was a “relaxation” procedure. Relaxation refers to a class of techniques that uses an iterative algorithm to approximate the solutions for a large system of equations. The techniques are also called “Gauss-Seidel” methods after Gauss, who pioneered the first such methods, and German astronomer Philipp Ludwig von Seidel (1821-1896), who extended the techniques. The idea was to start with the substitution of a trial solution into the first equation (the top row of the matrix), find the residual left by the trial solution and use that to calculate the next trial solution. Essentially the idea was to alter the values in the trial solution so that they would eliminate the residual just calculated. The standard approach would start with the top row and substitute each new trial solution into the row below through the entire matrix, generating a new residual. The matrices Smith and Eckert generated were exceedingly large. For example, 3500 equations in as many unknowns would be represented by a matrix of over 12 million elements. However, most of the elements in the matrix were at or near zero. They therefore decided to use a Southwell or successive over relaxation technique.\(^2\) Instead of blindly plugging trial solutions into all equations, they selectively relaxed those with the largest residuals. (Smith 1965, 30-41; Hoffman 2001, 64-66)

The use of the Southwell method was notable, because Southwell had advocated for it as a way of avoiding steps in the hand computation of solutions. The method was seen by some as too complicated to implement efficiently on computers since the operations, such as searching for the largest residual, could take as long as doing the evaluations. In Smith and Eckert’s implementation they set a marker or cut-off and only relaxed the expressions larger than this marker. After all the residuals were smaller than the marker, its value was shrunk by one bit and the relaxation was repeated. (Smith 1965, 38-41; Hoffman 2001, 65)

Initially Eckert and Smith attempted to simply solve the less problematic (small)

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\(^2\)The Southwell technique was named after its inventor, Richard Vynne Southwell (1888-1970), a British aerodynamics engineer. (Gay 2007, 277) He published a book on his relaxation methods in 1940. (Hoffman 2001, 776)
terms using the Southwell relaxation technique. The small divisor (large) terms would be solved directly by the inversion of an appropriate matrix. However, this initial approach proved unsuccessful because of the interactions between the terms solved directly and those solved by relaxation. Therefore a further step was required to eliminate the effect of some of the smaller terms. A slowly converging process was used to create a transformation matrix that made the problematic terms small. The transformation matrix also affected the terms to be solved directly and so the effect would have to be removed after the solution was derived. (Smith 1965, 41-43)

The basic form of the solution was to arrange the matrix such that it could be divided into four sub-matrices based on how they would be solved. The top right quadrant would be subject to elimination making its effect negligible, this allowed the top left corner to be solved directly, the bottom left corner would consist of already negligible elements that could be solved by substitution and the bottom right corner was solved via the Southwell relaxation procedure. However, the entire matrix was too large to be efficiently solved by computer. Therefore the matrix was further divided into sub-matrices based first on the terms with heavy interactions as suggested by considerations of the Delaunay variables and then divided again based on the size of the divisors. These sub-matrices were then subdivided into four for solution as previously explained. The goal was to reduce the number of equations in a sub-matrix to about 300-500, which was the limit of the computer’s fast memory. (Smith 1965, 42-44, 72-77)

The basic process of solution had been devised and implemented to a considerable degree by 1961. At this point most or all the work had been done on the IBM 650, a small machine originally released 7 years earlier. Some work was also done on an IBM 704, a larger machine oriented for scientific work developed as a successor to the IBM 701 in 1955. All this work had been carried out to 11 digits of precision. In 1961 work had begun to create programs for the IBM 7090, IBM’s first large transistorized machine finished in 1959. (Bashe et al. 1986, 171, 180, 448; Eckert and Smith 1961, 447-449;
Eckert and Smith 1976, 235-236)

As should be clear by this point, Smith and Eckert’s project did not consist of deriving one set of corrections to Brown’s old lunar theory. Instead several different values for the corrections were first derived by performing the process at higher precision and then further corrections were derived using the new corrected equations of position as the starting point for further corrections. The original equations of position from Brown’s theory became $x_0^3$, the residuals derived from these were $E_0$ and the corrected positions $x_1$. All this work had been carried out to 11 digits of precision. When Smith and Eckert moved on to the use of the 7090 they increased the precision. Eckert and Smith found that increasing precisions by two digits doubled the computing time. Therefore they used a sliding precision of between 12 and 14 digits for the various terms in the solution. They used the values of $A_\xi$ as a proxy to select high precision for small divisors. Products formed in the substitution and some terms from $\Omega$ were carried to a full 17 digits of precision. They used the same starting point of Brown’s theory ($x_0$), however the resultant residuals were labeled $E_1$. The substitution yielded 6325 equations in as many unknowns, almost double the number found in generating $E_0$. (Eckert and Smith 1976, 235-238, 243; Smith 1965, 51-52)

From the residuals $E_1$, three different sets of corrections were derived: $x_{2a}$, $x_{2b}$ and $x_{2c}$. The residuals for $x_{2a}$ (E:2a) and $x_{2b}$ (E:2b) were derived by modifying the equations from the initial approach, by introducing $q$, the ratio of the mass of the Earth-Moon system, to the mass of the Earth-Moon-Sun system.\(^4\) In E:2b multiplied the right hand sides of the fundamental differential equations (6.1-6.4) by $q$ and set the left hand sides to zero, and E:2a was $E_1+E$:2b. The different calculations were undertaken to examine the effect of including the term that had been neglected (set to zero) in Brown’s work. Brown

\(^3\)In their writings Eckert and Smith refer to various sets of corrected equations as $x_n$. This symbol refers to a complete set of equations of position and not to a single coordinate as normal convention might suggest. Eckert and Smith refer to this set of equations of position as the coordinates.

\(^4\)q=3.0494221 \times 10^{-6}
had reintroduced the term as a later perturbation after completing his basic derivation of the Moon's motion. The work also helped insure that the solution to find the corrections was converging. The difference $\epsilon = x_{2a} - x_{2b} - x_{2c}$ was calculated and found to be very small. This suggested to Eckert and Smith the robustness of the solution technique, because the residuals $E:2b$ were derived by using only the relaxation process. This was possible because $E:2b$ contained only 1446 terms large enough to avoid Eckert and Smith's cutoffs. (Eckert and Smith 1976, 235, 248, 259-260, 266-267)

Every newly generated set of expressions for the positions could be checked by simply substituting them into the differential equations to yield a new set of residuals. This could also be the first step towards a further extension, but not necessarily. After the derivation of the corrections from $x_{2...}$, Smith and Eckert derived residuals designated $E_2$ from corrections $x_1$ (work done on the IBM 650). This work showed that $x_1$ had achieved a great deal of precision, but Eckert and Smith judged it unsuitable for further refinement because of inadequate precision especially in the partial derivatives. $x_1$ was used as a benchmark against which to test later corrections. It had particular significance because the exact details of the method (such as the programs used) differed between $x_1$ and later solutions. In Eckert and Smith’s opinion, this gave it a significant role as independent confirmation of the work. (Eckert and Smith 1976, 236)

The remaining work using Airy’s method was done on various IBM 7094s. The IBM 7094 was another powerful transistorized machine released in 1962. Using these machines, Smith and Eckert derived a new set of residuals $E_3$ from position expressions based on corrections $x_{2c}$ and from these a set of corrections $x_3$. The residuals $E_3$ consisted of 6930 equations. However at this point it was decided to increase the precision still further. Thus these calculations were done in part to gauge the extent to which the precision of operations were affecting the final outcome. Residuals were again found for positions based $x_{2c}$, but in this case Smith and Eckert increased all the precisions. Now precision varied from 13-15 digits during substitution and derivation of the corrections used the
same criterion to determine which elements required higher precision as before. Also, 
the maximum precision taken for products and some other elements of the equations 
increased to 18 digits or the limits of a double-precision number on the 7094.\footnote{A double-precision number is one using two words of storage.} The 
residuals generated were designated $E_4$ and contained 9693 equations. This allowed 
them to gauge the effect of higher precision. (Bashe \textit{et al.} 1986, 449; Eckert and Smith 
1976, 235-238, 248)

The corrections $x_3$ were not precise enough for Eckert and Smith. Comparison of $E_3$ 
and $E_4$ suggested a strong dependence on the precision used in the calculations. They 
also discerned certain problems with $x_{2c}$ and $x_3$. They therefore decided to use $x_{2a}$ as 
the basis for their final solution. The residuals derived from $x_{2a}$ were designated $E_5$. The 
residuals $E_5$ generated 9672 equations, slightly less than $E_4$. The final set of corrections 
were designated $x_5$. (Eckert \textit{et al.} 1976, 248, 235-239)

Eckert and Smith were able to give preliminary findings in 1965. At this point cor-
rections $x_3$ had been generated as had residuals $E_4$ but not $E_5$ or $x_5$. The main cor-
rection derived from the calculation was $0''.072 \sin(2F - 2l)$ and this correction was soon 
checked. A reduction of the data from extensive observations taken over several years by 
researchers at the Naval Observatory gave a value for the perturbation as $0''.084 \pm 0''.025$. 
Again, as with the discovery of the error in Brown’s \textit{Tables of the Moon} due to work on 
the SSEC, an identification of an error in a theory allowed its empirical detection. (Klock 
and Scott 1965, 335; Smith 1965, ii-iii)

Although Smith’s thesis was completed in 1965 and results were presented in various 
venues around this time, the work continued for several more years. Smith continued to 
perform calculations on IBM 7094 computers in Europe, while on a Fulbright fellowship 
at the University of Madrid. He used machines at Imperial College in London and Lyngby 
in Denmark to complete the work. The definitive report on Smith and Eckert’s work 
would come as another monograph for the \textit{Astronomical Papers Prepared for the Use of
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the American Ephemeris and Nautical Almanac, volume XIX Part II of the series entitled The solution of the Main Problem of the Lunar Theory by the Method of Airy. Although given an official publication date of 1966, the actual date of publication must be later since Eckert’s death in 1971 is mentioned in the preface. Apparently Eckert completed the manuscript for the work shortly before his death. The 1966 date presumably reflected the original publication timetable. The library of congress catalogue gives a date of 1976 for the volume and this has been used as the date of record in this thesis. What caused the 10 year delay is unclear. As of 1965 only the calculation at $E_5$ and $x_5$ remained to be completed. The final publication notes that some terms could not be corrected, despite the great efforts expended on the attempt. Also, Eckert added extensive notes on his various refinements and analysis of Brown’s theory, including estimates of the motion of perigee, and node implied by the corrections $x_5$. The last of these publications on Brown’s theory was a reference dated 1969. (Eckert and Smith 1976, 189; Smith 1965, 46; Smith MS 2007)

In its final form Eckert and Smith gave the solution in rectangular coordinates to twelve decimal places of precision ($1 \times 10^{-12}$ about equal to an angular precision of $2'' \times 10^{-7}$) in latitude and longitude. In total, only one correction to Brown’s lunar theory was larger than $0''.01$, eight more were larger than $0''.005$ and 51 larger than $0''.002$. This again confirmed for Eckert that Brown’s solution had largely achieved its stated precision of $0''.01$ with respect to the main problem of lunar theory (ie not including planetary perturbations). The goal of the Smith-Eckert solution was to achieve a precision in the main problem of lunar theory of two orders of magnitude greater than that of Brown’s or about $0''.0001$ ($5 \times 10^{-9}$). The largest expected uncertainty that Smith and Eckert suggested for the coefficients in the new solution are of the order of $1 \times 10^{-8}$, showing that they felt they almost achieved their goal. However this also suggests that about 4 digits of the printed precision had little meaning. Among the reasons they stated for their confidence in the solution was the small number of corrections added in $x_5$ when
compared with corrections in $x_{3a}$ and $x_{3c}$. (Eckert and Smith 1976, 196-197, 259-262; Eckert and Smith 1966, 243)

Comparison with previous and intermediate results was an important part of Smith and Eckert’s justification and use of their results. In choosing the empirical parameters to use to evaluate their solution, Eckert continued to use those values agreed upon by Brown and Eckert in 1936. This meant that in order to achieve a solution that used the latest empirical parameters, a further set of perturbations needed to be calculated. Throughout the project Eckert made preparations to account for changes to the parameters. Another reason to continue with the same parameters was that, since 1938, the parameters were refined several times. Since Airy’s method solved only the main problem of lunar theory, the three body problem of Earth, Moon and Sun, perturbations for the effect of the planets, the shape of the Earth and other factors also had to be added. These issues meant that, while the improvements due to the Eckert-Smith solution could be confidently gauged by comparison to previous successful theories, their full accuracy was not readily available in the form of a set of definite positions for the Moon that could be compared directly with the new data generated by advancing technology, especially that associated with the lunar spacecraft missions. (Eckert and Smith 1976, 263-264; Mulholland and Devine 1968, 874-875) In fact Eckert makes no direct reference to such data despite the existence and publications of some results before the completion of the project.

One other achievement Eckert and Smith note for their efforts was on the question of the convergence of solutions in celestial mechanics and these specific methods in linear algebra. As discussed in Chapter 2, Poincaré had demonstrated that the convergence of the series used in celestial mechanics could not be guaranteed in strict mathematical terms. Similarly the methods of algebraic solution Smith and Eckert employed did not admit of rigorous guarantees of convergence. Therefore the consistency of their results with previous efforts gave them confidence in the continued relevance and applicability of their methods and of the reliability of new computer techniques. The evaluation of a
10,000 by 10,000 matrix was not common at the time and one memorializer suggested that Eckert and Smith’s solution exceeded the technology of the 1960s. (Smith 1965, 44; Eckert and Smith 1976, 195-196; In Memoriam 1972, 3)

In the partnership on the project to use Airy’s method, Smith was responsible for almost all the machine work. Smith would consult with Eckert on a regular basis for several hours going over results and discussing what to do next. Smith’s 1965 thesis focuses on summarizing the elements of this machine work. He developed a set of programs to be used iteratively to perform the necessary calculations. The programming used on the IBM 650 and 704 is not documented, but a great deal of information about the work on the 7090 and 7094 work is provided. Smith programmed the solution in the SHARE compiler-assembler-translator (SCAT) programming system for the IBM SHARE Operating System (SOS). Smith also made use of the 7090 Sort program. Using this software he developed over 20 programs consisting of 50,000 words of instructions. When run, the problem required significant operator intervention with the machine. The operator needed to handle the interchange between the various programs and also specify certain operations using the computer’s console switches. Finally, the large amounts of data recorded and processed were stored on tape and so an operator was required to change tapes as different operations finished. (Smith MS 2007; Smith 1965, 114-115)

The programs Smith created for the project tended to carry out a limited set of functions such as inputting a given set of data and then sorting it a certain way. The various programs were often used in slightly different variations and the need to do this was one reason that Smith chose the SOS programming system. The SOS system produced a “squoze deck”, a deck of punched cards with binary encoding containing in a compressed form the entire source code for the document. The operator could modify the program by including appropriate cards at the end of the program. Smith felt that it simplified the process of debugging and revising programs, presumably because modification could be achieved without reworking the entire program. Smith also felt
that, because many programs were used with slight modification at various steps of the process, it would be unwieldy to have a separate program for each variation. He preferred instead a single main programs run with modifying cards. (Smith 1965, 147-148) The obvious alternative would have been to create programs in a language such as Fortran, but this would require the recompiling of all the source code for each modification and maintaining a copy of each slightly modified program.

Examples of programs created by Smith included ZZ, a program to read in the values of z and calculate relevant values that depend on z; 5EDIT a program that takes 5 word output of other programs like ZZ and sorts and organizes it into a useful more permanent form. PURGE was a program to carry out the removal of the non-critical variables from the critical equations. SOLVE calculated a set of corrections once all the requisite terms were calculated, sifted and organized properly. The full solution required an iterative application of SOLVE along with various other programs to apply and properly organize each intermediate set of corrections. (Smith 1965, 114-119, 136-143, 147-149)

One feature of the program and solution, that shows continuity with Eckert’s earlier efforts, was the notation used to identify the various trigonometric terms. All the trigonometric terms in Brown’s series for the position of the Moon have arguments made up of the sum of various integer multiples of the Delaunay variables (F, l, l’ and D), for example \(\cos(F - 2l + 3l' - D)\). Therefore, Eckert and Smith identified each term in terms of the values of the integers in the argument, for \(\cos(F - 2l + 3l' - D)\) the integers are 1, -2, 3 and -1. In machine work these four numbers were encoded in a single 8 digit number, made up of four 2-digit numbers. Each 2-digit number was equal to one of the integers plus 50. So the term \(\cos(F - 2l + 3l' - D)\) would be identified by the numbers 51, 48, 53 and 49, or all combined 51485349. This method of identifying term by the integers used in its argument had been suggested and used by Eckert in his 1930s work multiplying and verifying Brown’s series. (Eckert and Smith 1976, 235; Eckert 1940a, 66-67, 100)
The storage requirements of Airy’s methods, with its 10,000 by 10,000 matrices, stretched the limits of the technology available. In order to efficiently fit the numbers in, Smith took advantage of the large number of zero value entries in the matrix. Repeated entries of zero were represented by a single word of storage indicating a repeated series of zero entries and the length of the series. This allowed matrices representing 300-500 equations to be stored in fast memory as opposed to the 160 equations that would have been allowed had all values remained explicit and uncompressed. This was important because, as previously mentioned, Smith and Eckert devised a complex subdivision of the matrix for solution, breaking it down into pieces that fit on fast memory. (Smith 1965, 73)

The need to create specific computing routines to deal with the problems of particular calculations is something Eckert took as a standard problem for computing. In a course syllabus for “Engineering 281 - Numerical Methods”, Eckert warned against the view that a few standard procedures would exhaust what was needed by the machine user. Eckert stated that numerical methods remained an art and a science and not just a matter of indexing standard solutions to a problem.

This concept is fine in the ideal, and the attempt to systematize machine processes is a profitable one. In real life, however, the day has not yet arrived when this process can be afforded to any great extent. (Eckert no date F, 2)

While clearly some progress had been made to the ideal of standardization the lengths Smith and Eckert went in streamlining for the task at hand suggested that careful tailoring remained necessary.

Despite the various economy measures undertaken, the solution still took a considerable amount of time. Smith estimated that substituting the initial solution and calculating the dependent values took 8 hours and that calculating the partial derivatives to find the equations to be solved took a further 20 to 25 hours. Another 25 hours were required to complete a solution. Smith estimated that the various iterations of the solution and testing took hundreds of hours. Smith and Eckert suggested that the slow speed and
linear access limitations of tape storage were a major factor in the length of time the computations took and that the introduction of computer systems with large-scale disk storage would alleviate this and significantly alter the logistics. Their rationale was that information on a disk could be accessed in a much more direct, or random, manner than linear storage on a tape. (Smith 1965, 114-115, 132; Eckert and Smith 1976, 236-237)

6.2 Eckert and The Hollow Moon Paradox

As mentioned previously, part of the initial impetus for Eckert to revive Airy’s method was the desire to increase the accuracy of the motion of perigee and node, which were given to less precision in Brown’s theory than the other parameters. The uncertainty of the terms was estimated, in 1932, to be on the order of $\pm 4''$ in node and $\pm 3''$ in perigee. One particular motivation to increase precision was that estimates of the motion of perigee and node, as given by Brown’s theory, left a large unexplained residual. It was thought that added precision might reduce or eliminate the problem. However some other possible causes of the residual were also considered. Among these Brown took advantage of the uncertainty about the figure (shape) of the Earth to suggest a value of the oblateness (flattening) of $\frac{1}{294}$ that reduced the discrepancy, but was different from the best current estimate of $\frac{1}{297}$. Also, later refinements by Brown and others, such as including higher order terms, refining the values of constants and correcting for approximations in the transformation to polar coordinates from rectangular coordinates, helped reduce some of the discrepancy. (Eckert 1965, 787-789; Jones 1932, 45-53)

Another factor that plays a role in the motion of perigee and node is the moment of inertia of the moon. This can be defined as a product of the mass distribution of the Moon and the angular velocity of the Moon’s rotation about its own centre. Assuming a more standard value for oblateness and given what was known of the Moon’s libration and

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6 The oblateness ($o$) is calculated by dividing the difference in length between the equatorial ($e$) and polar ($p$) radiiuses by the length of the equatorial radius, or $o = \frac{e-p}{e}$
oblateness, the implied distribution of the Moon’s mass became a problem for Brown’s theory of the Moon. Specifically it implied that the Moon’s density increased away from its centre, a configuration that came to be called the “hollow Moon paradox” by some commentators in the 1960s. Since in an object as large as the Moon, any such large concentration of dense material should sink to the centre quickly, this suggestion seemed to be ruled out by any reasonable physical constitution for the Moon. (Eckert 1965, 787; Kaula 1969, 1583)

In 1964, using his preliminary findings from his work with Smith, Eckert was able to establish new values of the motion of perigee and node attributable to the interactions of Earth-Moon and Sun with significantly greater precision (better by two orders of magnitude). He first reported on this in 1964 at the meeting of the International Astronomical Union along with the other preliminary results of his work with Smith. The change for motion of perigee was equal to about 8″ per century and this, along with other changes made for various reasons, resolved a significant discrepancy. On the other hand the value for motion of node was changed only about 2″. At the same time a new value for the oblateness of 1/298.25, measured by artificial satellites, further widened the discrepancy between theory and observation in the motion of node. This led Eckert to publish two papers on the motion of perigee and node and their relation to the moment of inertia and distribution of mass in the Moon, one in 1965 the other in 1967. A summary of work in lunar theory entitled “The Motion of the Moon.” dated 1967, for an internal IBM publication, also mentions the issue. (Eckert 1964, 113; Eckert 1965, 787-789; Eckert 1967a, 97; Eckert MS 1967b)

The later two published papers are almost identical in terms of technical content. The first “On the Motions of the Perigee and Node and the Distribution of Mass in the Moon” Eckert published in the Astronomical Journal where Eckert published much of his work. The second paper “The Moment of Inertia of the Moon Determined from its Orbital Motion” was published in a conference proceedings entitled Mantles of the Earth
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and Terrestrial Planets. The conference had occurred in 1966 in Newcastle upon Tyne. Both papers analyze the contributions to the motion of perigee and node from theory and compare the results with observations. The papers then go on to find values for the elements of the moments of inertia, in particular the density distribution, required to add to other theoretical terms to obtain the measured values of the motion of perigee and node. The analysis also considered the residuals left by other values for the distribution of mass and the other elements of the moment of inertia. The conference paper contained more context about the astronomical significance of the work and considered a slightly larger range of possible values for the elements of the Moon’s moment of inertia. (Eckert 1965; Eckert 1967a; Eckert MS 1967b)

The elements of the motion of inertia considered by Eckert were \( g' \) and \( f \). If \( A \), \( B \) and \( C \) are the moments of inertia of the Moon, \( r \) its radius and \( M \) its mass then
\[
g' = \frac{3}{2} \frac{C}{Mr^2}
\]
and
\[
f = \frac{C-B}{C-A}.
\]
The parameter \( g' \) related directly to the radial distribution of mass in the Moon, a value of \( g' = 0 \) indicates a point mass, \( g' = 0.6 \) indicates a homogenous body and \( g' = 1 \) indicates all the mass distributed at the radius (a hollow shell). A straightforward derivation by Eckert gave \( g' = 0.965 \) and \( f = 0.638 \). He also gave tables for the residuals in the motion of node \( (d\omega) \) and perigee \( (d\pi) \) left by various assumed values for \( f \) and \( g' \) (see table 6.1). Note that the motion of node depends only on \( g' \), but motion of perigee depends on \( f \) and \( g' \). (Eckert 1967a, 103-107)

The reason for highlighting these papers was the physical significance Eckert suggested for the numbers. The article in the Astronomical Journal is abstracted to a single sentence. “New calculations of the motion of the node and perigee of the moon, when compared to observation, indicate a large concentration of mass near the surface of the moon.” (Eckert 1965, 787) This abstract and Eckert’s decision to contribute to a conference on geological and selenological subjects suggest he believed his results had implications for the question of the physical structure of the Moon. The actual content of the papers however stress the many unknowns and the physical unlikely-hood of the
mass concentration suggested, referring to the issue as a discrepancy between a realistic lunar density and the observations of the Moon. Eckert points out that a homogenous density yields a smaller discrepancy than any other physically probable structure. The discrepancy between theory and structure for a homogenous Moon is 10" per century in motion of node. Therefore Eckert could have as easily summarized his first paper as indicating a 10" discrepancy rather than as a concentration of mass at the surface. (Eckert 1965, 787, 792) Indeed in the 1967 summary of lunar theory Eckert notes that “The trouble may be due to an oversight in almost any one of the links in the chain or it may be due to a cause presently unknown.” (Eckert MS 1967b, 12)

Eckert’s interest in this problem long predate these two papers. A hand written note on the manuscript of the 1930s results indicates a reference to a 1932 paper that claimed to have resolved the discrepancies in the motion of node. The origin of this note cannot be certain, since some parts of the document date back at least to 1943 and others are dated from 1958. Presumably the reference dates back to at least 1958 when Eckert returned to work in earnest again on the project. (Eckert MS 1943, title; Jeffreys and
A more direct reference to the issues appeared in a 1961 letter Eckert received from physical chemist and Nobel laureate Harold Urey (1893-1981). Although Urey was at the University of California in La Jolla circa 1961, Eckert and Urey had been colleagues at Columbia and apparently remained friendly afterwards. Herbert Grosch recalled a conference in 1959 on space exploration during which Urey and Eckert spent a great deal of time in conversation. In the letter of 1961, Eckert had asked Urey if there were any explanation possible for the Moon having a moment of inertia larger than that found for a uniform sphere (one that has mass concentrated away from the centre). Urey gave a reference to his own work on planetary formation from 1951 that might explain such a density. Urey also expressed interest in the results of Eckert’s calculation. (Urey MS 1961; Urey 1951, 229; Grosch 1991, 70, 84, 255-256) Eckert had long known that the motion of node, and the density it implied, might be a problem and was clearly considering the possibility that the hollow Moon density configuration was real.

Eckert’s choice to impute a physical significance to this phenomenon is the only example where Eckert himself takes up the challenge of using the motions of heavenly bodies to test novel theories about them. In giving motivations for pursuing astronomical research, Eckert had several times suggested that it allowed fundamental physical laws to be probed. He noted for example that “the classic problem [of celestial mechanics] remains one of our principal avenues toward a better understanding of the fundamental nature of things.” (Eckert 1957, 43) However, most of his work actually concerned production of ephemerides without drawing any broader conclusions about the world. The only other clear instance where Eckert drew broader implications from his work was in the case of the integration of the motions of the outer planets. In that case Eckert, Brouwer and Clemence argued for a mass of Pluto based on the results of the integration. Although implicitly the accuracy of all his work stood as confirmation of the physical theories and measurements of the parameters that underlay them.
Eckert’s conviction that the orbits of celestial bodies, such as the Moon and asteroids, present the possibility of probing the laws of gravity, was shared with and perhaps inherited from his mentor E. W. Brown. Around the turn of the 20th century it had been suggested that anomalies in the planetary orbits could be explained by reference to a slight modification of the inverse square law parameter. However this modification would lead to an alteration in the motions of perigee and node large enough to cause very large discrepancies between observations and Brown’s lunar theory, ten times larger than the discrepancy involved in the hollow Moon paradox. Also, Brown considered orbits as evidence of more subtle consequences of gravitational theory and the qualitative character of celestial motions, such as the stability of orbits. Brown’s motivation for studying the Trojan groups of asteroids was, among other things, to analyze issues of stability and to find resonance patterns. In a 1932 address to the American Astronomical Association, Brown also pointed to the distribution of the asteroids as evidence for resonance effects in the asteroids orbits due to Jupiter’s gravity. (Brown 1903; Brown 1923, 69; Brown 1932, 38-40; Doel 1996, 17)

Eckert’s statements, about using orbital trajectories as a probe of nature, refer explicitly to “laws” or “theories” not physical properties like density. These statements seem more in line with a development such as the use of general relativity to explain the motion of the perihelion of Mercury. Therefore it might seem incorrect to view this interest in the physical structure of the Moon as an example of this method. However, Eckert himself associates both sorts of discovery in the introduction to his 1967 conference paper on the Moon’s motion of node and mass distribution: “This comparison of observation and theory with ever-increasing accuracy has led to many discoveries including Newton’s law of gravitation and the irregular axial rotation of the Earth.” (Eckert 1967a, 97) In fact he ends his unpublished summary of lunar theory with musing about resolution to the hollow Moon paradox and notes the role of discrepancies in lunar theory in the discovery of the Earth’s irregular rotation. (Eckert MS 1967b, 12)
The association of such particular physical facts with universal laws is natural in astronomy because historically the discrepancies between theory and observation often leave open the possibility of positing new laws or a new physical structure. The motion of Mercury’s perihelion is a famous example of this. Some 19th century astronomers attributed the discrepancy between theory and the observed motion of the perigee to a new planet, Vulcan, inside Mercury’s orbit. The planet would never be found, but the motion of Mercury was explained by Einstein’s theory of general relativity. (Linton 2004, 441-445)

Historian Ronald E. Doel, in his book *Solar System Astronomy in America*, has identified the study of the physical structure of objects in the solar system, solar system astronomy, as a minor but going concern in astronomy, even before space exploration by artificial satellites, probes and manned spaceships became a reality. Doel focused his narrative on the formative period 1920-1960. Doel suggests that the collaboration between Brouwer at Yale and Eckert at Columbia in the 1930s was motivated, not only by the needs to use the asteroids as reference points for more accurate star charts, but also in order to study the origins of the asteroids using their dynamical qualities. Eckert’s research on the hollow Moon paradox fits well in Doel’s category of solar system astronomy, especially given Eckert’s consultation with Harold Urey about the formation of the Moon. Urey is a major figure in Doel’s narrative because of Urey’s extensive work on the chemistry of planetary formation. (Doel 1996, xi-xiii, 17-18, 91-106)

Eckert’s work can be connected to even older traditions in astronomy. John Herschel’s *Preliminary Discourse on the Study of Natural Philosophy* discusses the various elements of the methods of scientific investigation. One method Herschel makes note of was the method of residual phenomenon. He introduced it in the following passage:

Complicated phenomena, in which several causes concurring, opposing, or quite independent of each other, operate at once, so as to produce a compound effect, may be simplified by subducting the effect of all the known causes... and thus leaving, as it were, a residual phenomenon to be explained. It is by this process, in fact, that science, in its present advanced state, is chiefly
promoted. (Herschel 1851, 156)

Herschel goes on to cite various examples of the use of such methods. His first example was Encke’s investigation of a discrepancy in the period of a comet. Herschel noted that “Physical astronomy affords numerous and splendid instances of this [the method of residual phenomenon].” (Herschel 1851, 166) Herschel then pointed to the use of Newtonian gravity to explain planetary motion, by reference to effects of the sun first and then using planetary effects to explain residual motion. Eckert’s inferences about the internal structure of the Moon are of precisely of this form. Eckert subtracts all known causes of the motion in node from the observed motion leaving a residual motion to be explained. Eckert then sought the explanation for the residual motion in the structure of the Moon. (Herschel 1851, 166-167)

Eckert’s work can also be linked to the methodology Newton employed. Philosopher William Harper has argued that Newton’s extensive theorems in the Principia show a special and powerful methodology that Newton used. The theorems served not only to deduce and predict the motions of particular bodies, but also to examine alternatives to the proposed theory. In Harper’s analysis, this allowed Newton to consider elements of his theory as having parameters to be measured by considering accurate measurements of certain physical phenomena, such as the motion of perigee of the Moon or perihelion of the planets. Alternative values for the parameters would give different deviations from the measured phenomenon. These phenomena were not singular events like the position of the Moon at a given time, but were the average of behaviour over periods of time, these phenomena were subject to more accurate measurement and had greater generality. Harper notes these measurements are heavily mediated by theory. In fact Newton’s proposed theories are under Harper’s analysis “inferences from the phenomena” the identification and generalization of patterns in the observations of nature. (Harper 2008, 44-49)

An example of Newton’s method as identified by Harper is Newton’s arguments for
the inverse square law of gravitational attraction. Consequences of the inverse square law include both Kepler’s harmonic law relating the distance of the planets from the Sun and their periods and the absence of precession (motion) of the perihelion of a planet in the two body case. Newton showed that deviation from the inverse square law would lead to deviation from the harmonic law in the ordering of the planets and cause the perihelion of the planets to be subject to continuous change. Thus by measuring the precession of the planets and the relationship between their speeds and distances from the sun one establishes the inverse square law. Harper suggests that it is as if Newton considered gravitation to be a central force with acceleration proportional to $\frac{1}{r^n}$ and thereby measure the value of $n$ as equal to 2 by measuring the harmonic law and absence of precession. (Harper 2008, 46-49)

One other element of Harper’s analysis is his identification of Newton’s methodology as one of successive approximation. According to Harper Newton’s inferences from the phenomena were understood as approximations subject to revision. Specifically Newton’s method was one of successive approximation where deviations from the initial hypothesis served as evidence, new phenomena, for the necessary refinements to the model. In practice these refinements were often simply accounting for more gravitational perturbations, but held the possibility of modifying or falsifying the initial theory. (Harper 2008, 52-53) This is the method of residual phenomena again, but this time as a step in a larger method.

Harper identifies what he calls Newton’s method as an ongoing means of evaluating theories in astronomy citing as an example E. W. Brown’s 1903 work, mentioned above, showing the limits on changes to the inverse square law. Harper also suggests Einstein’s use of the perihelion of Mercury, to establish the superiority of general relativity, as another example of the ongoing use of this methodology. (Harper 2008, 53-55)

Although Eckert’s work in this case considered a physical property rather than a parameter of a theoretical law, Eckert still seemed to be employing what Harper calls
Newton’s method. As already mentioned, these sorts of physical parameters are intimately related to possible evidence for different laws. Also, the moment of inertia of the Moon was not independently measurable by other means at the time Eckert wrote and so had much of the character of a theoretical parameter. In any case, Eckert’s writing on the hollow Moon paradox had the character Harper ascribes to Newton’s methodology. Eckert considered a range of possible values for the moment of inertia and how they agreed or disagreed with the measured motion of node and so measured the moment by finding the value of it which would reduce the discrepancy to zero.

The space exploration efforts of the United States and Russia sent probes and (in the case of the USA) men to the Moon. These efforts allowed the distribution of mass in the Moon to be measured by far more direct means. These measurements indicated that the Moon had no concentration of mass near the surface, but rather is almost homogenous with a slight increase in density towards the centre. (Goudas 1967, 955-956) At the same time the increased analysis and scrutiny of the Moon, by scientists, led to a possible resolution of the paradox. One researcher, T. C. van Flandern (1940-2009), of the US Naval Observatory, suggested that the observed value of the motion of the node had not been corrected for the change in the period of the Earth’s rotation over the periods considered. This suggestion was first reported by William M. Kaula (1926-2000) in 1969 as the resolution of the “hollow Moon paradox”. However, this would turn out to be only part of the answer. When van Flandern released the recalculated values in 1970, the change to the observed motion of node was an addition of only 4″ per century, leaving at least 6″ of motion per century unaccounted for. With the advent of later more accurate lunar theories, this 6″ discrepancy would be attributed to larger than expected errors in the planetary perturbations. The actual planetary perturbations amount to about a 7″ change, but this was counterbalanced by minor changes in the motion, due to the main

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7Kaula refers to the problem explicitly as the “hollow Moon paradox” with quotation marks and is my source for the term.
problem and other factors, and a 1″.9 motion attributed to relativity. Strangely this relativistic effect seems to have been known to Eckert but discounted, on the advice of Clemence, as already included in the effects of precession. (Kaula 1969, 1582-1583; van Flandern 1970, 246; Sôma 1985, 76; Eckert 1965, 788)

I could find no explicit and complete resolution of the paradox Eckert had posed in the late 1960s or early 1970s. Eckert, himself, never published anything that retracted or refuted the statements he made. As a result, one obituary of Eckert mentioned his speculation about the Moon’s structure as one of his accomplishments and Gutzwiller was unable to provide any insight into the resolution in his biographical sketch of Eckert. (Ashbrook 1971, 207; Gutzwiller 1999, 157) Yet, it seems that many of the researchers of the day had concluded that Eckert’s findings were in error soon after his publications on the topic. In a 1969 letter, J. Derral Mulholland (c.1934-) of the JPL, dismissed the density distribution Eckert had derived as “experimentally discredited.” (Mulholland MS 1969) This suggests that the new measurements by space probes had convinced astronomers that Eckert’s calculation indicated an error in some part of the lunar theory Eckert used. In 1968 Mulholland and others at the JPL reported on larger than expected errors in a lunar ephemeris based on Brown’s theory and in this report they speculated that the planetary terms were the main cause of the discrepancies. The same report included estimates by Gerald Clemence that the unaccounted for contributions from planetary terms might be as large as 0″.1 in latitude and longitude of position, ten times more inaccurate than the accuracy Brown had sought for the main (Earth-Moon-Sun) problem. (Mulholland and Devine 1968, 874-875) Eckert himself had focused all his work on the main problem of lunar theory and he never addressed in any details errors in other elements. Therefore astronomers like Mulholland and Kaula may have surmised that the error, introduced by Brown’s planetary terms, was large enough to eliminate the force of the discrepancy.

Also, as mentioned, Eckert never evinced a deep commitment to the hollow Moon
configuration. While he seems to have viewed it, at least briefly, as a viable physical option, he may always have viewed it as somewhat improbable. The intent of his papers may have been as much a goad, to encourage further research into the issues, as a serious claim about lunar structure. Indication of this is seen in his summary of lunar theory where he states: “For the time being, we must accept the paradox of the motion of the node as a stimulant to further study.” (Eckert MS 1967b, 12) In this regard Eckert’s papers succeeded, at least according to Mulholland’s 1969 letter: “This work generated considerable interest in the observed elements of the lunar motion and in the determination of the density distribution as reflected through the gravity field.” (Mulholland MS 1969) On the other hand Eckert’s willingness to consider the possibility of the unlikely hollow Moon density distribution may represent his commitment to his methodological convictions. The possibly largely rhetorical character of Eckert’s pronouncements may explain the lack of a concerted effort on his part to dissolve the paradox as new information emerged.

6.3 Eckert’s Refinements to Lunar Theory and the Mission to the Moon

In the final years of his life Eckert continued work to improve lunar theory. In addition to the work already described, Eckert also carried out work to maximize the accuracy that could be obtained from Brown’s theory. Brown’s theory was still the basis for the lunar ephemeris and so the standard against which new theories of the Moon and new more accurate observations were compared. Therefore this work was done both in terms of recalculation of the basic series used by Brown and as corrections to the existing numbers calculated for the ILE and national ephemerides of the Moon.

In 1966 Eckert co-authored a paper in the Astronomical Journal entitled “Transformation of the Lunar Coordinates and Orbital Parameters” with his wife Dorothy Eckert
and M. Judy Walker. Brown had calculated his solution of the main problem of lunar theory in rectangular coordinates, but the natural observational coordinate system is polar coordinates relative to the Earth. Therefore Brown had transformed his solution into polar coordinates. Also, Brown had calculated subsidiary planetary and other effects in polar coordinates and not in rectangular form. Finally Brown introduced a transformation to allow the calculation of latitude to be done based on the tables for the longitude. The various transformations and additions created rounding and other errors and Brown had neglected terms in sine parallax (radial distance from the Earth) because of the relative inaccuracy of measurement of it and its unimportance in the astronomy of the day. However, the new space missions promised more accurate measurement of sine parallax and the accurate prediction of sine parallax was a necessary component of lunar missions. The ILE, calculated on the SSEC, had been based on the complete series for the theory as brought together in Brown’s publication of the Tables with all these limitations. This new paper, by Eckert and his coauthors, consisted of a recalculation of the transformation to polar coordinates from Brown’s original series in rectangular coordinates, published in Brown’s original theory work. This series had also been the basis of Eckert and Smith’s implementation of Airy’s method. Also Eckert sought to update the equations in terms of the new astronomical constants set by the IAU. (Eckert et al. 1966, 314)

The work required a careful numerical transformation of the long harmonic series using computer. At least some of the actual machine coding was done by one J. S. Rosenbaum and not the Eckerts or Walker. Also the effects of some the parameters had to be recalculated in more precise ways in order to maintain the higher precision demanded. Eckert and his coworkers also compared the intermediate calculations and results to the values from the Eckert-Smith lunar theory. This helped establish the precision of the transformation and help calibrate the new transformation as well as helping to characterize the differences between the Eckert-Smith solution and Brown’s
solution. The corrections, due to the new transformation, to sine parallax amounted to as much as $0''.012$ or about 1400 meters for a given time. Also given in this paper was a set of corrections to allow users to update the ILE values already calculated, rather than having to recalculate the entire series. (Eckert et al. 1966, 314, 322, 323, 325)

Even before this new transformation was completed, the lunar ephemeris had been recalculated with the new parameters and taking into account the large correction found by Eckert and Smith’s work. The ephemeris on the same basis as the ILE was designated \( j=0 \), and the new values were designated \( j=1 \) and used as the new standard for the published lunar ephemeris by 1968 and until 1972. A version of the ephemeris incorporating the new transformation (as well as previous corrections) was designated \( j=2 \) and was used in the National Almanacs starting in 1972. An ephemeris largely equivalent to \( j=2 \) generated by the JPL and designated LE 4 was in use circa 1968. (Eckert et al. 1969, 473; Mulholland and Devine 1968, 874)

Several accounts of Eckert and his work state the Apollo missions to the Moon relied on data taken from the ILE calculated on the SSEC. (Gutzwiller 1998, 624; Bashe et al. 1986, 57) Cortada’s biographical sketch provides an example: “Twenty years later, the results of his calculations were used to determine the path of the Apollo space ships.” (Cortada 1987, 85) Such statements are at best an oversimplification, since the ILE was not the direct source of the data used by NASA. A similar but more accurate statement appears in The IBM Watson Laboratory at Columbia University: A History by Jean Ford Brennan:

Fifteen years later, “The Ephemeris” [ILE] and subsequent results which appeared under the titles “The Transformations of the Lunar Coordinates and Parameters,” and “The Solution of the Main Problem of the Lunar Theory by the Method of Airy” were to constitute the basis for the orbital calculations of the NASA Moon programs: Surveyor, Lunar Orbiter and Apollo. (Brennan 1971, 23)

The JPL efforts built upon the work of Brown and Eckert rather than being directly derived from them.
The letter, previously quoted from Derral Mulholland, summarizes Eckert’s contributions to the American space exploration program. The letter was in response to a request from IBM about the accomplishments of Wallace Eckert and the appropriateness of granting him an award in recognition of these accomplishments. Mulholland explained that Eckert had shared his results with the JPL before publication, allowing them to update their ephemeris. He identified the transformations as Eckert’s most important contribution to the space program. Eckert’s work did contribute to the Surveyor, Orbiter and Apollo programs, however, Mulholland qualified his statements as follows:

Inevitably, as requirements have become more stringent and observations more precise, we have isolated further inadequacies in the lunar theory and have produced ephemerides that are more accurate over current time spans. This has benefitted immensely from the previous work of Dr. Eckert. Indeed without his prior work, significant portions of my own work would have been much longer in coming, if not impossible. (Mulholland MS 1969)

Mulholland identifies the Apollo Laser Ranging Experiment as a project he was involved in that benefited from Eckert’s work. (Mulholland MS 1969)

Publications by JPL scientists on lunar theory, reveal that they developed accurate numerical integrations of lunar motion to refine predictions as of 1968. Also, the information from lunar orbiters and landers, gave new and far more accurate measurements of the Moon’s surface. The final ephemeris used in the Apollo program included refinements from these sources and so would be significantly modified from Eckert’s work. Errors as large as 0″.16 (300 m) in longitude, 0.12 (225 m) in latitude and 0″.0047 (500 m) were found by the JPL in their LE 4, when compared with radio data from the lunar Surveyor 1 probe that landed on the Moon, and a numerical integration LE 5. However, LE 5 depended for calibration on values taken from LE 4, showing the dependence of the new work on the old. Note again that the inaccuracies found in Brown’s theory are much larger than the 0″.01 precision that Eckert claimed for it. This is because Eckert only concerned himself with the main problem of lunar theory and not subsidiary issues such as the planetary perturbations which seem to be playing a large role in these er-
errors. (Mulholland and Devine 1968, 874-875) All this supports the conclusion that Eckert made substantial contributions to the US Space program, but also suggests the limits of his contributions.

It is difficult to determine why exactly Eckert chose to focus most of his attention on the main problem of lunar theory when other aspects had become an impediment to precise calculation. However, the three-body problem, which is at the heart of the main problem of lunar theory, holds a special place in developments of classical mechanics because of its difficulty. As mentioned in Chapter 2 of this thesis, Poincaré developed his radical analysis of the limits of predictability of planetary motions while considering the solution to the three-body problem. Also, the orbits of all of the major planets and many asteroids can be calculated from Newtonian theory with considerable accuracy, without truly engaging the three body problem. Instead the effects of bodies other than the Sun are considered as perturbations on the Keplerian elliptical motion. The Moon required a much more complex solution, dependent on finding approximate solutions to the three body problem, to achieve similar levels of accuracy. Therefore the problem of the Moon’s motion became almost synonymous with the three body problem.

The complexity of the task Eckert had set for himself is suggested by a mistake discovered in Eckert’s work. In preparation for the change to the new standard for the lunar ephemeris, T.C. van Flandern at the US Naval Observatory and G A. Wilkins at the Greenwich Observatory, discovered a discrepancy between the value of the new series derived by Eckert and his coworkers when calculated directly and the values given by correcting the old ILE values. The discrepancy amounted to $0''.034\sin (F-2D)$. The first suggested cause of the discrepancy was an ambiguity in Brown’s formulae that had been dealt with differently in the ILE and in the new transformation. The ambiguity had been noted by Woolard in his comparison of the ILE and Brown’s tables. However, Eckert showed that the cause of the discrepancy was the use of a coefficient with insufficient precision in the new transformation. This error was not made in the values used to correct
the ILE, hence the discrepancy. A report on this error was published by Eckert, Wilkins and van Flandern, where they also established that Brown’s series had been incorrectly implemented in the ILE. In fact the ambiguity noted by Woolard had led to the ILE being incorrectly calculated. The mistake in the new transformation by coincidence negated most of the error in the ILE created by the ambiguity. (Eckert et al. 1969, 473-477; Morrison and Sadler 1969, 133) This incident again suggests the difficulty of reproducing the results of large human computation efforts.

6.4 Eckert’s Unfinished Lunar Theory

As work on the Eckert-Smith lunar theory by the method of Airy wound down, Eckert began his last ambitious project. When Eckert had described Airy’s method in 1940 he noted its advantages over even a partially literal method such as Brown’s as follows: “when used in connection with a good literal theory it [Airy’s method] gives result which could be obtained by the literal method alone only at the cost of tremendous effort.” (Eckert 1940a, 98) However, by 1967 Eckert had decided to undertake the tremendous effort and derive a new more accurate (partially) literal lunar theory using the Brown-Hill approach. Two papers discussing the plans were published, one in the Astronomical Journal entitled “The Literal Solution of the Main Problem of Lunar Theory” and co-authored with his wife Dorothy Eckert. The other publication was a 1973 conference proceedings of a Moscow conference on celestial mechanics and astrodynamics in March of 1967. Comments indicate that Eckert began his work, with the aid of both Dorothy Eckert and M. J. Walker, suggesting that planning for this project began while working on the transformation of lunar coordinates and parameters. Also, Walker ceased being a member of the Lab’s technical staff in 1966 meaning that work must have begun no later than 1966. This would explain how extensive calculation had already been undertaken by 1967. (Eckert and Eckert 1967, 1299; Eckert 1973, 66; Brennan 1971, 61)
Eckert’s intent was to develop the new literal theory to 12 digits of precision for comparison with the Eckert-Smith solution. However, as previously mentioned, the uncertainty in the calculation of that solution was characterized as being as high as 1 in $10^{-8}$, even though all components and the solution were given to 12 digits. Presumably Eckert meant that the theories component terms should be developed to 12 digits of precision, but acknowledging that the overall precision of the series would be significantly lower. Eckert suggested the new solution would not only serve to further confirm physical theory, but again would serve as a useful contribution to applied mathematics, establishing the precision of another large series solution. (Eckert 1973, 66)

Eckert’s proposal had several novel elements beyond simply pursuing higher precision in the Hill-Brown method. In the earlier method several elements of the forces, depending on the ratio of the Earth-Moon system’s mass to the Sun’s and the ratio of the Moon’s mass to the Earth’s, were solved separately from the rest of the main problem and added as separate perturbations. Eckert felt that: “Aesthetically, it is desirable to solve the entire “main problem” by a single unified method.” (Eckert 1973, 66) For the purposes of comparison, he proposed solving the equations both with the terms included and in the simplified manner of the traditional Hill-Brown method. He also proposed solving the equations for three values of $m$ (the ratio of the Moon’s mean motion and the Sun’s) using the simplified (original) equations and three values of the ratio of masses of the various bodies for the complete equations. Eckert argued this would allow interpolation of the solution to different values of these parameters with high precision. This would be especially important for $m$, whose numerical value is substituted in at the beginning of the solution. (Eckert and Eckert 1967, 1299)

The Hill-Brown method begins with the basic differential equations of lunar motion
and rewrites them into the form:

\[
\begin{align*}
D^2 u + 2mDu &= -\frac{2}{(n-n')^2} \frac{\partial \Omega'}{\partial s} \\
D^2 s - 2mDs &= -\frac{2}{(n-n')^2} \frac{\partial \Omega'}{\partial u} \\
D^2 z &= \frac{2}{(n-n')^2} \frac{\partial \Omega'}{\partial z} \\
D &= \frac{1}{i(n-n')} \frac{d}{dt}, u = x + iy, s = x - iy \\
m &= \frac{n'}{n-n'}, q = \frac{E + M}{m' + E + M} \\
\Omega' &= [(E + M)/r] + \Omega + \frac{1}{2}n'^2(x^2 + y^2)
\end{align*}
\]

(Eckert and Eckert 1967, 1300) where \(r, x, y\) and \(z\) are the positions of the Moon relative to the centre of the Earth with axises moving with the mean motion of the Moon, \(n\) is the mean motion of the Moon, \(n'\) the mean motion of the Sun, \(m'\) is the mass of the Sun, \(E\) the mass of Earth, \(M\) the mass of the Moon and \(\Omega\) is the acceleration of the Earth and Moon due to the sun. The method of solution is to consider successive approximations to the equation. \(\Omega\) is given by a series solution and an additional term in \(\Omega\) is included as the equations are solved for higher orders. The variational orbit is also called the zero order solution and this solution can be derived from these differential equations with only one term in \(\Omega\). (Eckert and Eckert 1967, 1299-1301)

In the case of the variational orbit the differential equations take the form:

\[
D^2(us) - DuDs - 2m(us - sDu) + \frac{9}{4}m^2(u^2 + s^2) = 0 \\
-\frac{3}{4}m^2q(3u^2 + 2us + 3s^2) = C \\
D(us - sDu - 2muds) + \frac{3}{2}m^2(u^2 - s^2) - \frac{3}{2}m^2q(u^2 - s^2) = 0
\]

(Eckert and Eckert 1967, 1301) where \(C\) is a constant related to the total energy of the system and \(i\) is the square root of minus one. The equations for \(z\) disappear for this case. The equations of Hill set the \(q\) term to zero, this is sometimes termed an assumption that the mass of the sun is infinite, because the mass of the sun \((m')\) is in the denominator of
q. Also, \( p \) is a term depending on the masses of Earth and Moon that appears in higher orders of the equations and is also set to zero in the original Brown-Hill method. Note that, since \( u \) and \( s \) are complex conjugates of each other, the value of one can be derived for the other in a straightforward manner. Therefore the solution is generally found in terms of \( u \) and \( z \). (Eckert and Eckert 1967, 1300-1302)

The solution for the variational orbit and all other orders of the solution is given as a series of harmonic terms. As in the Smith-Eckert solution all harmonic terms have as their argument some combination of the Delaunay variables \( D, l, l' \) and \( F \). The series has the form:

\[
 u = a \sum A_{j\beta\gamma\eta} e^{i(jD + \beta l + \gamma l' + \eta F)}
\]

\( j, \beta, \gamma, \eta = 0, \pm 1, \pm 2 \ldots \)

\[
 D = (n - n')(t - T_0), \quad l = c(n - n')(t - T_1),
\]

\[
 l' = m(n - n')(t - T_2), \quad F = g(n - n')(t - T_3)
\]

(Eckert and Eckert 1967, 1301)\(^8\) where \( a, c, g \) and \( A_{j\beta\gamma\eta} \) are functions of the ratio between the Sun and Moon’s orbits and the mass terms \( p, q \) and the parameters of the motion \( e, e', k \) and \( \alpha \). As with \( \Omega \) \( c \) and \( g \) are given by series approximations and at successive orders of the solution successive terms in \( c \) and \( g \) are introduced. (Eckert and Eckert 1967, 1299-1301) Each term \( A_{j\beta\gamma\eta} \) consists of the sum of the following form:

\[
 A_{j\beta\gamma\eta} = \sum C_{n_1,n_2,n_3,n_4} e^{n_1} e^{n_2} k^{n_3} \alpha^{n_4}
\]

(Eckert and Bellesheim MS 1976, 15) where \( e \) is the eccentricity of the Moon’s orbit, \( e' \) the eccentricity of the Sun’s orbit, \( k \) is the inclination of the Moon’s orbit, \( \alpha \) is a function relating the mass of the Moon and Earth and the average motion of the Sun, \( n_1-n_4 \) are integers varying from 0-9 in value\(^9\) and \( C_{n_1,n_2,n_3,n_4} \) terms are functions of \( m, p \) and \( q \).

---

\(^8\)The integers \( j, \beta, \gamma \) and \( \eta \) were designated \( i, p, q, \) and \( r \) by Eckert and Bellesheim. To avoid confusion with other terms that use the same letters I have changed them.

\(^9\)\( n_1-n_4 \) were designated \( a, b, c \) and \( d \) by Bellesheim, again to avoid confusion I changed the designations.
The integers $n_1$, $n_2$ and $n_3$ are equal to the absolute values of $\beta$, $\eta$ and $\gamma$ respectively and $n_4$ is even when $j$ is even and odd when $j$ is odd. The coefficients $A_{j\beta\gamma\eta}$ can be characterized by the powers $n_1$, $n_2$, $n_3$ and $n_4$ they contain (a power 0 indicates the absence of a parameter) and the combination of $e$, $e'$, $k$ and $\alpha$ they contain is called the characteristic. For the zeroth order or variational curve $e'$, $k$ and $\alpha = 0$ (or rather $n_2, n_3$ and $n_4=0$) in all coefficients. The sum of the powers $(n_1, n_2...)$ of the characteristic give the order of the term, except for the zero order (contains one power in $e$). So the coefficients of terms in the first order contain a characteristic consisting only of $e$ or $e'$ or $k$ or $\alpha$. Also, in the first order the $k$ is only found in $z$ and is the only parameter present as a factor in the coefficients of $z$. At higher powers $z'$s characteristic contain factors of $\alpha$ in addition to $k$ and $k$ is found in the characteristics of some terms of $u$. (Eckert and Bellesheim MS 1976, 14-15, 64; Eckert and Eckert 1967, 1301; Gutzwiller and Schmidt 1986, 33)

The solution was found by considering the sums of the coefficients of like terms (terms with the same argument) and setting those sums equal to zero creating variation equations. Also terms found at previous orders were removed. Most such variation equations were solved in pairs. However in some cases small divisors in the coefficients prevented convergence of the pair wise method and instead they were solved directly in 4 or 5 pairs using matrix methods. (Eckert and Bellesheim MS 1976, 51-54).

Wallace Eckert and Dorothy Eckert laid out the basic plan of solution for this method in the 1967 paper. In the same paper they re-derived the zero order solution using the modified method and multiple values for $m$ and of the mass ratio. The three values of $m$ chosen were Hill’s original value ($m_1$), one equal to Hill’s value plus 3 times $10^{-9}$ ($m_1 + 3 \times 10^{-9}$) and the third adds the same amount again ($m_1 + 6 \times 10^{-9}$). All values of $m$ were to 15 digits as Hill had done. The second of these is very close (within 2 times $10^{-11}$) to the IAU 1964 value for $m$. This was supposed to allow for easy adjustment within that range due to any update in the value of $m$. Similarly the values for the ratio
of Sun’s mass to the Earth-Moon’s is varied by a unit in the third figure with the middle term close to the value set in 1964 by the IAU. The re-derived values for the parameters of the zero order variational curve, agreed with Hill’s to 15 decimal places or more in all cases when the work was reduced to the simpler case. Also re-derived were the zero order contributions to motion of node and perigee. (Eckert and Eckert 1967, 1304-1307)

In addition to Dorothy Eckert and Judith Walker, Wallace Eckert was also aided by Sara Bellesheim in this work. Walker had prepared part of the program for the IBM 1620 to solve the zero order equation and Bellesheim had run the problem on the computer. The work was programmed in Symbolic Programming System (SPS). The programs added and multiplied series and found their derivatives and conjugates all to 20 digits of precision. Note that the IBM 1620 was a small and inexpensive computer for its day. (Eckert and Eckert 1967, 1305, 1307) As with his approach to Airy’s method, Eckert began this work on a small scale. At the same time he continued his pattern of pushing the limits of the technology.

Bellesheim, like many of Eckert’s assistants, had no training in astronomy before she began working for Eckert. He trained her in the various skills required to set-up and solve the equations. Her ability to carry the work on after Eckert’s death, illustrated the extent of the training she received. (Gutzwiller MS 1976) By this point Eckert had a great deal of experience training assistants.

Although Eckert retired from the Watson Lab in 1967, he continued to conduct research at the lab as an IBM Fellow and only retired his Columbia professorship in 1970. After they completed the initial work in 1967, Eckert continued to work with Bellesheim, to construct the solution to the lunar theory at the higher orders. When the Watson Lab at Columbia closed in 1970, Eckert and Bellesheim moved their work to the Watson Laboratory at Yorktown Heights. Eckert grew ill shortly after the move. Martin Gutzwiller, then director of the Watson Lab at Yorktown Heights, took over supervision of Bellesheim at this time. Gutzwiller was not trained as an astronomer and worked in quantum theory.
However he had an interest in the relation between classical and quantum mechanics and had discussed various issues in classical mechanics with Eckert. Thus, when Eckert died on August 24th, 1971, Gutzwiller continued to support Bellesheim’s work and offer her advice. Bellesheim’s position at the Laboratory was apparently imperiled as Bellesheim began to work part-time while caring for her young children. (Gutzwiller 1999, 147; Gutzwiller MS 1976; Tropp 1978, 128-129)

At the time of Eckert’s illness, work on the solution up to the 2nd order had been completed. Eckert had set the goal of determining all terms to the sixth order and Bellesheim managed to complete this by 1975. In 1976 Bellesheim, with Gutzwiller’s assistance, prepared a manuscript for the NAO including some selections of drafts Eckert had written for various purposes. Eckert had arranged for the NAO to publish the new solution before his death as part of Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac. However, after several more years and a comparison by Gutzwiller of the results with other new lunar theory, Bellesheim’s work was not published. Instead Gutzwiller teamed with Dieter S. Schmidt who had independently worked out a lunar theory based on the Brown-Hill method. Together they produced a new solution and published it as The Motion of the Moon as Computed by the Method of Hill, Brown, and Eckert. The addition of Eckert’s name was in honour of his work, but their method deviates from the one he had advocated and planned. (Gutzwiller MS 1976; Gutzwiller 1979, 889; Gutzwiller and Schmidt 1986)

Bellesheim’s work after Eckert’s death was extensive. Eckert had decided that all terms, up to and including the sixth order, should be calculated and included and that precision should be 18 digits for the zero order solution terms and one less digit for each successive order. This represented a massive increase in precision from Brown’s calculation. Brown had only included a few digits of those terms from those sixth order terms he had included. Also, Brown had not systematically calculated all terms and instead had decided whether to include and calculate terms based on the size of their coeffi-
The total number of terms is never stated but the tables would have contained 1292 groups each with an average of 16 results listed, this suggests the series consisted of 20,000 terms. However, the tables listed coefficients to identical functions but different characteristics that would naturally be combined, therefore the effective number of terms for a calculation are in the range of a few thousand at most. (Eckert and Bellesheim MS 1976, 42; Gutzwiller 1979, 891-892) Eckert apparently felt that, as with the extra terms in mass he included in the main calculation, these extra small terms in the solution could be handled by the computer at little extra effort and would add an aesthetic completeness to the procedure.

The terms in the theory can be identified by their characteristic and the Delaunay variables that make up their argument. As in his other lunar theory work, Eckert chose to represent the argument in machine readable form by 4 two digit numbers, each two digit number is the integer, by which one Delaunay variable in the argument should be multiplied, plus 50. The characteristic is represented by four single digits from 0 to 9, one for each of the powers of the parameters $e$, $e'$, $k$ and $\alpha$. To these 12 digits were associated the coefficient derived in the solution. (Eckert and Bellesheim MS 1976, 35)

The computer work for the first order terms were also done on the IBM 1620, programmed in Fortran II. Bellesheim coded the programs for work on the higher orders, on the powerful IBM 360/91 at Yorktown Heights, using Fortran IV. This Fortran IV code had a standard precision of 8 digits, in order to achieve the precision required for the computations, Bellesheim coded routines in assembler for triple-precision (24 digit) arithmetic. These routines included addition, multiplication, division of normal precision integer values to give a triple-precision product and various routines to convert to and from triple precision representation and store values as well as check sums. (Eckert and Bellesheim MS 1976, 43-55) This work marks the only time that the popular scientific programming language Fortran is used in published work Eckert supervised. The need to code their own triple precision arithmetic code suggests that Eckert and Bellesheim
were working at a precision rarely seen in scientific computation and were continuing to stretch the available tools to their needs.

As Eckert had hoped, the basic coding and structure was reusable at various orders of the solution above the first. However each new order of coding required various alterations. Also, as mentioned previously, terms that would not converge, when considered in pairs, would have to be solved directly. In some cases Bellesheim deemed it advantageous to solve a series starting with the terms that would require the most iterations and proceeding on to one’s requiring fewer steps. The mechanism to input this ordering was a punched card. The large series solved by the programs necessitated the use of large amounts of storage including fast core, intermediate disk storage and also magnetic tape. (Eckert and Bellesheim MS 1976, 43-55)

As mentioned previously, in 1979 Gutzwiller published a comparison of the lunar theory produced by Bellesheim with other theories. Gutzwiller dubbed the Eckert-Bellesheim effort the “Eckert Lunar Ephemeris” or ELE. This naming convention goes back to the Improved Lunar Ephemeris, dubbed ILE, and the later analytical lunar theories against which Gutzwiller compared Eckert and Bellesheim’s work shared similar naming schemes. A comparison with Brown’s original theory confirmed the Eckert and Bellesheim solution as consistent with those calculations, but this would be expected as Bellesheim had checked her work against Brown’s figures as she worked. Indeed results were set to be displayed with the difference between the new calculation of the coefficient and Brown’s coefficient included. However, comparison of the prediction of motion in perigee and node, indicated a larger difference (10'' in a century for both terms). This was attributed to the large difference in the component c since there was disagreement on the 4th digit between Brown’s terms and the ELE’s value. Brown’s number was given to 5 digits while the ELE’s numbers given to 15 digits. (Gutzwiller 1979, 891,)

Gutzwiller noted that the comparison with Brown’s theory, as modified later by Eckert, proved complicated because of a change in the variables used by Eckert for those
adjustments. Instead Gutzwiller took advantage of the fact that the later lunar theories developed in the 1970s had already been compared to the ILE and decided to simply compare these newer theories and ELE values for the coordinates. In order to make the comparison Gutzwiller tested the tolerances of the series to the smaller terms. He established a cut-off, eliminating the set of smallest terms that altogether added to \(0''.01\) in longitude and latitude and \(0''.00001\) in sine parallax. This left 609 terms in longitude, 530 in latitude and 585 in sine parallax. This truncation limited the precision of the theory to about \(0''.001\). (Gutzwiller 1979, 893, 896)

The first theory with which the ELE was compared was the Analytical Lunar Ephemeris, ALE, completed circa 1971. ALE had been developed by pursuing Delaunay’s wholly analytical approach to a new level of precision and accuracy. The comparison of the ELE with ALE consisted in comparing the coefficients of the various common terms, looking only at those large enough to give a meaningful difference. This showed differences on the order of \(0''.001\) in terms of longitude, the largest difference being 0''.0063 with many more an order of magnitude smaller. Comparable differences were found for latitude, but overall differences were smaller. This agreement was far closer than that found comparing ALE to the ILE, where differences were an order of magnitude greater. The comparison of ILE and ALE did not include sine parallax, but the largest differences Gutzwiller found between the terms in ELE and those in ALE in sine parallax were \(0''.00089\). The calculated values of the motion of the perigee and node indicated differences of 5'' in century for both values, also indicating better agreement between ELE and ALE than between the ILE and ELE. (Gutzwiller 1979, 891-892, 896-897)

The second theory Gutzwiller compared the ELE against was the Semi-Analytical Lunar Ephemeris, SALE, published around 1979. This theory began with Hill’s variational orbit, but pursued a different method from that of Hill-Brown to find perturbations to a more accurate solution. While the maximum difference in the terms of longitude between ELE and SALE was still \(0''.00051\), the remaining terms were often much smaller than
there counterparts in the earlier comparison and suggest that the differences in ELE and ALE were often due to round-off errors in the ALE numbers. The improvement in latitude was much less pronounced, but sine parallax also saw an improvement despite having a slightly larger highest difference of 0′′.000090. Finally the derivatives of the terms in the ELE and SALE agreed to a remarkable extent. Still Gutzwiller noted about 24 terms in the SALE that would be equivalent to terms of the seventh or higher order in the solution method of the ELE, i.e., terms left uncalculated in the actual ELE, and these had a noticeable impact in the comparison. (Gutzwiller 1979, 897-899)

The deviations in latitude and longitude given by Gutzwiller’s study suggest that the accuracy of the theory relative to other work was of the magnitude 0′′.001. Gutzwiller did not compare any of the resulting data against observations directly. This parallels Eckert’s analysis of his own work that focussed on comparisons between theories to a large extent. So the implied improvement in the ELE from the ILE is about an order of magnitude (0′′.001 versus 0′′.01). This precision seems less than Eckert and Gutzwiller might have hoped for and also the comparisons had shown that some seventh and higher order terms would play a significant role. Ultimately, Gutzwiller felt that the improvement in measurement and other lunar theories demanded a more accurate solution to the main problem\(^{10}\) leading him to combine efforts with Dieter Schmidt rather than publishing the Eckert-Bellesheim ELE. (Gutzwiller and Schmidt 1986, 11) This work would significantly deviate from Eckert’s original plan and will be detailed in the conclusion.

Strangely Gutzwiller does not appear to have ever made a comparison between the ELE and the Eckert-Smith solution. This despite it being Eckert’s explicit intention to do so at the outset of this project. It seems likely that the ALE was compared with the

\(^{10}\)Note that the ELE, ALE and SALE only solve the main problem of lunar theory and so could not give accurate positions of the Moon without additional terms for planetary perturbations and other secondary effects. Also all three are formulas for computation and not lists of daily position, despite the use of the term ephemeris.
ILE as modified by Eckert’s subsequent work, including the Eckert-Smith conclusion. In which case, Gutzwiller may have felt that the ALE was already a standard superior to the Eckert-Smith work.

Eckert’s final work was in many ways his most ambitious. Thirty years earlier, the project had seemed too onerous to undertake. He clearly had a sense of the potential for computer aided algebra in lunar theory. However, the methods remained a work in progress, and so he and his assistants were forced to innovate with the materials available. This innovation was less dramatic than Eckert’s earlier use of punched card machines or one-of-a-kind machines, but still a part of a larger pattern in his work. It is interesting that in this work he had taken a more principled approach of including terms on the basis of the character of the solution (the order of the terms) rather than a more ad hoc basis (the size of the coefficients) that had traditionally been used by Brown. This suggests an optimism about the utility of the mature and fast computing technology that existed in his later life and the behaviour of the equations. Despite these efforts, Gutzwiller and others would still find themselves led back to a need for a more ad hoc rules by the requirements of computation.

As seen in this chapter Eckert’s work from 1957 onwards is marked by its focus on Lunar theory. He pursued three separate approaches during this period. He began by once again utilizing Airy’s method, this time with IBM’s modern computers, to create a new more accurate lunar theory by iterative correction of Brown’s basic lunar theory. He also sought to capture the full accuracy of Brown’s theory by rederiving its transformation to spherical coordinates. Finally he sought to derive a completely new lunar theory using the Hill-Brown method, redoing and extending past what Brown had done. These methods demonstrated the power and limitations of the machines and techniques at Eckert’s disposal.
Chapter 7

Conclusion

The aim of this thesis has been to examine the career of Wallace J. Eckert and, in the process, examine the relationship between the computer and scientific practice. In chapter 2 we saw that computation has long been carried out by humans with the aid of tools, some as basic as writing and others more complex. The long tradition of computing in astronomy makes for an obvious continuity between Eckert’s work and that of his predecessors.

In addition to the more general elements of an emphasis on numerical methods and the invention and adoption of mechanical calculating machines by scientists in the late-nineteenth and early twentieth century, a few specific methods that would dominate Eckert’s career and at the same time be transformed by his work should be noted. First numerical integration, specifically Cowell’s method, had grown in popularity during the 1910’s and 20’s. In the 1930s Eckert used punched card machines to partially automate the work of doing these calculation and in so doing both increased the use and range of these methods and the use of punched card machines in science. His work on the 1950 SSEC numerical integration of the motions of the outer planets made numerical integration, for the first time, the standard of accuracy for the position of any major planet. Despite these significant successes Eckert did not make use of numerical integration in
any of his later work.

Instead his later work was focused on the lunar theory of his mentor E. W. Brown. In fact his work on lunar theory began in the late 1930’s as a collaboration with Brown on an attempt to verifying and improve Brown’s lunar theory. This attempt involved the use of a method pioneered by Airy in the late 19th century. Airy had been unable to bring his method to fruition in how own lifetime, Eckert using punched card machines was able to verify Brown’s theory but not improve upon it at that time. However Eckert returned to the use of Airy’s method in the late 1950’s and with the aid of modern computers he and his assistants were able to carry the work to completion after more than a decade of work.

At the same time Eckert worked to improve upon Brown’s lunar theory in other ways. In 1948 he began a project to calculate the Moon’s position directly from Brown’s theory avoiding simplifications found in the standard way of calculating such positions using Brown’s *Tables of the Motion of the Moon*. In the 1960s he further improved Brown’s theory by recalculating the transformation from the rectangular coordinates in which the theory was initially formulated into the spherical polar coordinates of observation. Finally in the later years of his life, Eckert set out to rederive lunar theory using the Hill-Brown method to a new level of precision, using computers. Even though he had originally justified using Airy’s method because it avoided just such a long rederivation.

The dominance of earlier methods and theories in Eckert’s work suggests his conservative nature. However, his use of them illustrates how the changes in computation method changed the status of these methods. Numerical integration went from being a specialized method employed in problematic cases to a new standard. Airy had originally failed to yield useful results with his method but Eckert was able to yield important novel results with it in the 1960s assisted by computers.

Eckert’s decision to employ Airy’s method and Hill-Brown theory to improve on Brown’s theory rather than numerical integration is also notable. It illustrates, on the
one hand, a conservatism about employing a new method. This was despite his success with automating numerical integration in the 30’s and 40’s. On the other hand, the move suggests Eckert’s recognition that computer methods could be used to automate algebraic and analytical operations as well as purely numerical ones. Finally it suggests the role of choice in deciding what methods and applications computers should use. Eckert’s choice argues against the view that methods were simply dictated by the capacities of his equipment, even though the success and efficacy of Eckert’s methods clearly depended on his machines.

Finally this thesis has attempted to deal with how Eckert’s work changed technology and the company IBM. In the 1930’s and 40’s Eckert stretched the limits of the technology of his day and blazed a trail that others would follow. The support and cooperation of IBM was a vital part of Eckert’s success in this endeavour and so Eckert’s research agenda was shaped by his need to garner that support. The most obvious example of this was Eckert’s work communicating the new computing techniques he and others developed. At the same time IBM was receptive to his ideas about how their machines could be used for science.

All these conclusions will be discussed in more depth in relation to the literature on computer methods in the history and philosophy of science throughout the rest of this conclusion. Before the discussion of how my analysis of Eckert’s work related to the literature in the history and philosophy of science, it is necessary to discuss the immediate consequences of his efforts in celestial mechanics in the final decades of the 20th century.

7.1 Lunar Theory and Celestial Mechanics After Eckert

As will be clear from previous references, Eckert was not alone in attempting to create a new analytical lunar theory. The late 1960s saw the origins of what would eventually
become three other analytic theories. The first of these was the ALE. ALE was developed by André Deprit (1926-2006), Jacques Henrard (1940-2008) and Arnold Rom (1933-) at the Boeing Scientific Research Laboratories, Seattle, WA. They pursued Delaunay’s approach of solving the Moon’s motion with all parameters kept as unknowns (literal) during the development of the approximate solution. Their work included considering various techniques by which to carry this out. The actual algebraic techniques used were not those of Delaunay but were instead optimized for the computer methods used. They began presenting their results in 1971. (Deprit et al. 1971a, 257-258; Deprit et al. 1971b, 271)

Deprit had actually sought Eckert out as a reference when the Boeing laboratory shut down in 1970. Deprit was able to get a postdoctoral research position at NASA in 1971 and thanked Eckert for his reference and support. (Deprit MS 1970; Deprit MS 1971) Henrard from this group would go on to produce the SALE. Henrard came to work at the University of Namur in Belgium by the late 1970s and early eighties when he carried out this work. SALE began with an analytical treatment of Hill’s variational orbit. Henrard then created an improved solution using the differences between the nominal momentum function and the mean momentum function in each direction to correct the solution. Results for the Main Problem were published in 1979 only a year after Henrard had published a paper on solving Hill’s orbit. Further papers by Henrard dealt with other aspects of determining lunar position. (Henrard 1979, 337-338; Henrard 1978, 195)

Despite the use of the word ephemeris, only one complete ephemeris of the Moon based on analytical methods was completed after Eckert’s death. This is the Éphééméride Lunaire Parisienne (ELP). The ELP was the work of Michelle Chapront-Touzé and Jean Chapront, a wife and husband team. The initial impetus was work done by Jean Chapront and L. Mangeney in 1969 on an analytical ephemeris with provision for iterative approximation of more accurate solutions. The work has the character of Airy’s method and the Chapronts have called their own method and Airy’s “semi-numerical” methods.
Chapront’s initial approach was unsuccessful because of slow convergence, but Michelle Chapront-Touzé was able to build on her husband’s work and develop an alternative approach that succeeded in giving the required precision. This work was done during the seventies starting by 1974 and including Chapront-Touzé’s 1976 PhD thesis with a report of success in 1980. (Chapront and Mangeney 1969, 425-426; Chapront-Touzé 1982, 54-57; Chapront-Touzé 1980, 86)

A combined effort of Jean Chapront, Michelle Chapront-Touzé and other researchers, added in planetary and other perturbations to the solution, in work reported in 1982 and 1983. All stages of the calculation were carefully checked against the ephemerides of the JPL derived by numerical integration. Chapront-Touzé and her coworkers calculated various solutions with different designations. The most straightforward distinction is between ELP 1900 and ELP 2000. The ELP 1900 was constructed using measurements for the epoch around 1900, following Brown, Eckert and others, the ELP 2000 used updated measurements for a later time frame. The ELP 2000 actually consists of several versions for example ELP 2000-82 was the first standard developed and the ELP 2000-85 was used to derive values for ancient lunar positions. In 1984 the ELP 2000-82 became the basis for the ephemeris, _Connaissance des Temps_, of the French Bureau des Longitudes, where the Chapronts worked. The ELP in modified form continues to be the basis for the _Connaissance des Temps_. In the ELP the Main Problem was solved to great precision, but the planetary terms imposed a limit on the accuracy such a solution could attain. The errors introduced by planetary perturbations could not be reduced in the analytical theory to below 0′′.01 in longitude, even thought the main problem was solved to greater accuracy. (Chapront-Touzé and Chapront 1983, 50-53; Chapront-Touzé and Chapront 1988, 342; Engvold 2006, 6; Gutzwiller and Schmidt 1986, 50-51) This left numerical integration as the means to get the best accuracy possible in lunar theory by achieving better accuracy for the planetary effects.

Dieter S. Schmidt has already been mentioned. He began the work of Brown to a
new level of precision independent of Eckert and Bellesheim’s efforts. His main work on this was published in 1979 and 1980 followed by his collaboration with Gutzwiller on a reworking to greater precision of the Hill-Brown method. The work of 1979-1980 was carried out at the University of Cincinnati. He also argued that the analytic ephemeris required confirmation against each other because checks of internal consistency were not sufficient guarantees of reliability. Schmidt also credited André Deprit for many of the ideas about computer algebra used in these efforts. (Schmidt 1980, 135-136, 143)

Schmidt and Gutzwiller’s lunar theory differed from the original ELE\(^1\) in that based inclusion of terms on their contribution (coefficient size) not their order in the method of solution. Many terms that were included in the original ELE were not included in the new theory because they did not contribute and merely complicated the solution. On the other hand, some terms of the 9th order had to be included because of their contribution to the solution. The original ELE was used to check the solution they derived as far as possible, but otherwise the two works were independent. Gutzwiller and Schmidt compared their work with the latest analytic lunar theory, the ELP, and found agreement to the last significant decimal for all but 4 terms three of which differed by 1 unit in the last place and the other by 2 units. This established both the Gutzwiller-Schmidt theory and confirmed the ELP’s precision in terms of gravitational theory to \(10^{-10}\) or \(0.00001\) in longitude (the coefficients of each term therefore were individually good to perhaps \(10^{-12}\)). (Gutzwiller and Schmidt 1986, 11-12, 37, 42-43, 49-50)

There are some other difference between Eckert’s original plan and the Gutzwiller-Schmidt solution worthy of note. One was the decision not to attempt to solve for multiple values of \(m\) or other parameters. Apparently this proved too cumbersome to implement and also was replaced by simply taking the derivative of the solution with respect to the parameters. The other difference to note is that, while Eckert and his

\(^1\)The solution of Schmidt and Gutzwiller would itself be called the ELE in later literature. (Gutzwiller 1998, 628)
assistants were forced to devise and implement their own schemes for solution of linear equations, Gutzwiller and Schmidt could rely on pre-existing computer assisted algebra packages and routines to do such work. (Gutzwiller and Schmidt 1986, 31, 70)

At the same time as these developments in analytical theory, numerical integration of the Moon’s orbits had made enormous strides. As previously mentioned, the JPL had developed numerical integrations for guiding space vehicles to land on the Moon. As of 1970 Mulholland and other scientists at the JPL had developed “Lunar Ephemeris [LE] 16” a lunar ephemeris based on a combination of numerical integration and analytical approximations. The work previously mentioned had shown the need to use numerical integration to attain precision in all the motion due to gravitational attraction of the planets, Sun and Earth. However, they were unable to give an adequate treatment of tidal forces and the effects of the figure of the Moon in numerical integrations at that time. They therefore relied on analytical techniques to incorporate these empirically measured, but unpredictable effects. The hybrid ephemeris achieved accuracy on the order of 100 meters and proved itself superior to previous analytic ephemerides for high-precision work. The JPL would continue to develop lunar ephemerides based on numerical integration that strove to increase accuracy. In 1984 the JPL’s LE 200 became the basis for the tables of the Moon in the tables published by the United States and United Kingdom’s national ephemerides. (Garthwaite et al. 1970, 1133-1134; Gutzwiller 1998, 630; Standish 1982, 297)

Numerical integration also became an important tool for investigating the more qualitative issues in celestial mechanics, such as the stability of the orbits of the planets, a questions discussed in the history of astronomy by the likes of Laplace and Poincaré. A notable example of this was the Digital Orrery project. This project by MIT scientists and engineers, led by G. J. Sussman (1947-), created a purpose-built computer designed to quickly perform numerical integrations, using Cowell’s method, taking advantage of parallel operations by having 10 separate processors connected to a central
command computer. Among the results the team derived was evidence that the orbit of Pluto is chaotic, subject to radically different outcomes based on slight changes in initial conditions, when considered on the time scale of tens of millions of years. The evidence adduced for this was based on running numerical integrations covering periods of hundreds of millions of years and noting how the trajectories evolved, their sensitivity to initial condition changes and mathematical characteristics. (Applegate et al. 1985, 822-823; Sussman and Wisdom 1988, 433-436) This use of a specially designed machine recalls the early days of machine computation and computers and the project seems as much an exercise in machine design as in astronomy research.

The successes of numerical integration and its greater accuracy call into question the need for analytical solutions. Authors such as Schmidt, Chapront-Touzé and Chapront have admitted the greater accuracy of numerical integration but argued for the importance of analytical methods in any case. Schmidt suggested three reasons to use analytical theories, first the ability to achieve greater accuracy for calculations covering a large range of dates, second the ability to recognize the periods of perturbing functions and third the ease of considering the effects of the parameters on the solution. Chapront-Touzé and Chapront similarly argued that analytical methods allow better analysis of the components of a model on its overall long term predictions and produced an analytical ephemeris for just such a long time series. (Schmidt 1980, 135; Chapront-Touzé and Chapront 2000, 34) The merits of such arguments are beyond the scope of this work to judge, but the decision of whether to use analytical methods or numerical integration is not as clear cut as it had been when the absence of high speed calculation made analytic methods seem superior. The fact that the Anglo-American ephemeris makers chose one method while the French Bureau des Longitudes chose another suggests that national, personal or particular disciplinary interests may were drivers of the choice after the advent of high-speed machines. One researcher at the Bureau des Longitudes in 1995 described it as “the world recognized centre in analytical celestial mechanics.” (Brumberg
This statement suggested both the institution’s chosen area of specialization and pride as a possible motivation for that speciality. The institutional rearrangement of astronomical computation brought about by the computer and other developments deserves its own study.

Of course the two approaches need not be seen as at odds with one another. Work using both techniques continues to this day, both in deriving actual positional predictions and in studying the more general properties of celestial bodies and their motions.

Eckert himself did important work using both numerical and analytical methods, but despite Eckert’s use of numerical integration in the 1930s and 40s, he chose to use more analytical methods to obtain improvements to lunar theory. Eckert himself judged that, in the 1950s, machine methods had done more to revolutionize numerical integration (special perturbations) than analytical theories (general perturbations). (Eckert 1959, 149) Apparently in the mid-sixties it was not clear which would prove superior for high-precision work. Eckert’s choice to pursue an analytical approach may have signaled Eckert’s personal attachment to the work carried out by his mentor Brown and others in constructing analytical solutions.

Eckert’s later work in lunar theory marked significant changes from his earlier work. Whereas before he was operating on the frontier of technology, first with punched card machines and then with the SSEC, in his work of the 1950s and 60s, Eckert and his group made do with standard computers, often small ones. However the novel and ambitious uses to which he put these more pedestrian machines, recalls his earlier pioneering spirit. The increase in computing power created by the computer led him to more ambitious work. Sometimes this ambition was evident in the desire to reach new levels of accuracy, but it also comes out in his willingness to try techniques and methods that had previously seemed impractical. His ambitions seem to have run up against the limits of the science and technology at his disposal, with work running past deadlines and not quite achieving the desired accuracy.


telly0s of the ancients often required innovations in calculation such as Ptolemy’s table of trigonometric cord values and Kepler’s early adoption of logarithms. Even geometric theories created problems for their users in terms of calculation. The derivation of the orbits of the planets from Newtonian theory created more problems in calculation. Newton’s failure to derive an accurate value for the Moon’s motion of apogee and d’Alembert, Clairaut and Euler’s successes demonstrated how the limits of calculation had become an important factor in understanding the implications of theory. The need to derive orbits for comets and asteroids created a new set of problems in celestial mechanics, generating standardized methods of solution. With the establishment of national ephemerides new institutions of dedicated computers were created.

Alan Grier, in his book *When Computers Were Human*, has done a great deal to document these cultures of calculation that developed during the modern era especially, in the second half of the nineteenth and first half of the twentieth century. Eckert and his projects feature as an example in Grier’s work. Eckert shares with Grier’s other subjects a specialized set of skills in performing large calculations. Eckert’s network of fellow researchers were, like him, specialists in computation: researchers such as E. W. Brown and Comrie who encouraged him in his work. This perhaps helps explain how someone as soft spoken and self-effacing as Eckert became a dedicated promoter of machine computation. Much like several of the projects Grier detailed, most of Eckert’s assistants who actually carried out calculations, with the aid of machines, were women. Although Eckert always credited these assistants for their contribution it seems that, like the woman Grier profiles, they could not pursue a career in academic science as senior researchers. (Grier 2005, 192-194, 208)

The question of continuity speaks not only to Eckert’s social, disciplinary and institutional roots, but also to the nature of changes that occurred with computerization. It
is common to talk of a computer revolution in science. Douglas S. Robertson titled his book *Phase Changes: The Computer Revolution in Science and Mathematics* directly invoking the idea of the computer as creating a radical discontinuity in the practice of science. Computer historian, Jon Agar has recently responded to this notion of a computer revolution. He questions whether changes in science are ever revolutionary. Agar further suggests that the rhetoric of revolution blunts discourse and investigation into the process, nature and factors of change. He also calls doubts claims that certain developments were impossible without the computer. He puts forward a conjecture, a direct challenge to the idea of a computer revolution, that all computer methods in science were not novel, but had direct precursors done by hand, or early punched card machine. (Agar 2006, 872-873)

My own investigations of Eckert’s work are certainly consistent with many of Agar’s claims. Indeed Eckert’s work in organizing numerical integration, reduction of tabular data and Airy’s method on punched card, constitute more examples of the punched card precursors Agar cites in his paper. Similarly I would agree with his assessment that the adoption of computers depends on the network of relations scientists find themselves in. The list of users of the SSEC shows that scientists with a connection to the Watson lab were far more likely to use it than unaffiliated scientists. Eckert’s connection to IBM was the determining factor in his use of punched card machines in the 1930s and his easy access to machines and computers after 1945. As Agar found for his examples, the benefits of computers and other devices for an application were weighed by Eckert in terms of cost and speed relative to result rather than possibility or impossibility. (Agar 2006, 897-900)

Agar points to the division of labour in science with respect to computation and claims that computers deepened the division and expanded professional computing. Clearly my analysis of astronomical methods in general supports the view that computation labour was specialized before the computer and continued to be so afterward. However, at least
in a discipline as specialized in computation as celestial mechanics, it may have softened
the divisions between computational and non-computational practitioners and reduced the
need for professional computing. For example, the 1910 return of Halley’s comet required
the work of Crommelin, Cowell and the Greenwich observatory’s research staff, while
the 1986 return was found by a single JPL researcher far more accurately and well in
advance. (Agar 2006, 898; Grier 2005, 121-123, 319-320)

While I am broadly sympathetic to Agar’s claims, I would still contend that some
radical change in science has been dependent on the computer. In order to counter his
assertions I would first qualify the extent to which the same methods are used on the
computer and in hand computations or punched card computations. As I discussed in
Chapter 5, Eckert used a different formula for numerical integration on the SSEC than the
one used for asteroid orbits in the 1930s. To some extent this change might be relatively
immaterial (mathematically at least), like the decision not to use a formula containing
differences. However other changes, such as the calculation at each step of new positions
for all the outer planets, affected the outcome of the calculations and significantly added
to its complexity. Also, the use of a numerical integration to give the standard almanac
values for 5 of the major planets was a notable break with past practice.

Similarly, in order to say that, for example, Airy’s method was done by hand and by
punched card before it was done on computer, you must gloss over some of the details.
Neither of the attempts made before Eckert and Smith’s in the 1960s was carried to
completion. Airy gave up his attempt by hand unfinished. Eckert’s first attempt resulted
in a set of residuals but a new solution was never calculated from them. In a similar
vein, Eckert had not wanted to undertake the work required for a new more precise literal
theory according to the method of Brown, until fast computers adept at algebra offered
an easier approach.

Of course, Agar’s claim is not that no change resulted from the introduction of the
computer, but rather no revolutionary change, or wholly new method, resulted from
the adoption. Agar admits that qualitative changes were achieved in analytical power by taking advantage of efficiencies of time and money created. He gives as an example the extension of analysis in x-ray crystallography with the advent of more computing machinery. (Agar 2006, 898) The crux of the issue then is what or how much change would constitute a revolution. Agar quotes Robertson’s definition of revolutionary change, what Robertson calls a phase change, that consists of two elements: “that any extrapolation of the previous behaviour in the field will completely fail to give an accurate picture of the field following the phase change” and “the phase change is characterized by a novel ability to see things that could not be seen prior to the phase change.” (Robertson 2003, 9) Agar himself seems open to the possibility that some developments in computers have allowed scientists to see and detect previously unknown things and come to new understandings, but wants to deny discontinuous change. (Agar 2006, 871, 899)

However, Robertson’s claim of revolutionary change does not require change to be discontinuous, rather it requires the change to be unpredictable, that before the computer the field had one character and that after it had a different character, unheralded by the earlier state. The presence of intermediate stages between the two states is not fatal to the claim that the change could not be anticipated, since the emerging pattern may not be clear in the intermediate period. One minor but clear example of unexpected developments in computing was Comrie’s decision to compute parts of the position of the Moon out to the year 2000 in his 1928 endeavour, anticipating that any change to lunar theory would be given as corrections to Brown’s tables. Not only was this prediction refuted by Eckert’s *Improved Lunar Ephemeris* in 1954, but the underlying assumption, that scientific computation would continue to be carried out with extensive use of tables, was, if not rejected, under serious attack in 1954.

Agar’s strategy of refuting claims of revolutionary change by demonstrating intermediate developments has another problematic feature. He assumes that the intermediate development can be ascribed to the pre-revolutionary period. Instead we can argue that
the intermediate steps show that the revolution consists of various elements and each, in its own right, may be a revolution. Surely developments involving intermediate machines like the ENIAC and SSEC fall more on the side of computers given their electronic speed and versatility. In many cases, the earlier developments Agar points to as refuting claims of revolutionary change, involve the use of punched card machines to perform some complex computational task. For example, botanical survey efforts in Britain were mechanized on punched card in the 1950s and not computerized until the 1970s. (Agar 2006, 878-879) However, punched card machines already exhibit some key features of later computers and it would be dubious to maintain a sharp distinction between the accomplishments of computers and punched cards.\(^2\)

For example, as I noted in Chapter 3, the ability of punched card machines to encode data, in a discrete (digital) machine readable form, allowed a new ease of copying and manipulating data by machine. This new capability allowed a new practice of sharing to appear, a small revolution based on the appearance of a new technology. The use of computers saw this practice extend with greater complexity as discussed in Chapter 5. Not only could more fine grained results such as those of the numerical integration of the outer planets be shared via punched card, but the actual programs used could be shared as digital copies as Neil Block requested from Eckert in 1960. This sort of digital copying allowed a sharing of effort not previously seen and an ability to reproduce and understand previous results that was often difficult, if not impossible, to achieve. In recent years the ability to easily copy software has been an important part of software development for the personal computer, including the appearance of the “open source” software movement. (Campbell-Kelly and Aspray 2004, 213-217, 270)

Agar claims that talk of revolutions discourages investigation of the circumstances and process of change. On the contrary, claims of revolution have tended to focus historiographic attention on the supposed revolutions, and explanations of these revolutions

\(^2\)This line of thinking was first suggested to me by Isaac Record.
abound. Agar’s claim of orderly gradualism in science might, on the contrary, paper over important changes that occurred. In particular it is worrying that Agar’s attempts to evaluate whether some achievement was impossible without the computer, fail to evaluate what was possible given the actual constraints of resources and time. Instead Agar seems to refer to purely ideal constraints on what might have been given enough resources and time. Given “world enough and time” the Church-Turing thesis assures us that a hand computation can reproduce any computation done by machine. However, such a scenario seems to be of mostly philosophical import and not of much concern to historians and sociologists dealing with actual events.

The rise of numerical integration in the later half of the 20th century, as the standard method for calculating planetary ephemerides, gives an example of what may be a revolution due to the computer. In 1984 the American and British Almanac offices began publishing tables based on a new standard in their jointly produced Astronomical Almanac. The JPL’s new Developmental Ephemeris (DE) 200, and subsequent revisions, would form the basis of the printed positions from 1984 of all the planets in the Astronomical Almanac. This new ephemeris was based on numerical integration and replaced the older numerical integration of the outer planets and traditional analytical approximations used for the inner planets. Note that some of the theories of the inner planets replaced by the DE 200 were based on the work of Newcomb almost a century earlier. The French almanac, Connaissance des Temps, chose to employ new analytical theories for the derivation of all planetary positions, despite lower absolute accuracy, as it had done in lunar theory. (Dick 2002, 533; Brumberg 1996, 89-90)

The reason that this development constitutes a revolution is the change it signals in astronomers’ expectations and habits. Numerical integration or mechanical quadrature had been a conceivable strategy to calculate planetary solution since the time of Newton. However, the intense labour involved had diverted researchers to attempt to find analytical approximations that would avoid such prolonged and possibly futile drudgery.
The expectation was that such an analytical method would require less work and be as, or more, reliable and accurate. Complex analytical theories were labour saving devices, carefully crafted to achieve a balance of accuracy and speed. The invention and development of computers changed the view of the relative efficacy of the two methods. To obtain the necessary accuracy with the new methods was seen as more trouble than numerical integration.

Of course the continued use of analytical ephemerides in *Connaissance des Temps* and the slow piecemeal adoption of numerical integration show this revolution was neither sudden nor totally inevitable. I would not want to claim that other factors played no role in the adoption of numerical integration, or its success in achieving theories of greatest accuracy. On the contrary, I have attempted to trace some of those other factors in my thesis. However, the computer still seems a necessary component in the story.

### 7.3 Negative Reactions to the Computer

Donald MacKenzie’s book *Mechanizing Proof* spends some time detailing reaction of the community of mathematicians to the use of a computer proving the four colour theorem. The key point is that many mathematicians were somewhat incredulous about the reliability of the proof. Even more strongly, some mathematicians claimed that such a proof was bad mathematics, or no proof at all, lacking essential virtues such as giving insight to the problem. (MacKenzie 2001, 137-149) By comparison, no one directly challenged Eckert’s work because it was done by computer, or earlier by punched card. The most likely explanation of this is the low status of arithmetic among mathematicians and long use of low status assistants and mechanical aids to perform computation. Therefore there was no expectation that this sort of mathematical work would contain insight or that its reliability was a reflection of the reliability of the mathematician.

Even though Eckert was apparently never attacked for using the computer, some have
attacked the use of computers in the physical sciences more generally. One such attack is given by applied mathematician Clifford Truesdell (1919-2000). Although he admitted computers had proved useful to work in mathematics and physics and were necessary for some work in physics, such as high speed orbit computation, he extended the criticism leveled against the computer proof of the four colour theorem to the physical sciences. He warned that the uncontrolled use of computers risked abandoning standards of rigour in solutions and heuristics that insured the reliability of results. He also raised the danger of false theories being made to seem true. For example, a computer could calculate the epicycles necessary to make a Ptolemaic astronomy observationally accurate. (Truesdell 1984, 595, 607, 620) Truesdell’s worry is partially echoed by philosopher Paul Humphreys who raises the same scenario, but in contrast Humphreys is generally quite sanguine about computers and their use in science. (Humphreys 2004, 133-135)

There have been some general attacks on the use of the computer in astronomy. The lament of Schlomo Sternberg, that numerical integration carried out by computer had displaced humans from an ancient profession, is one. It suggests that he felt there was a special value that human dedication to celestial mechanics brought. A similar tale of disillusionment is given by Herbert Grosch about his University of Michigan PhD supervisor Allan Douglas Maxwell. Maxwell eventually went to work at the US Naval Observatory in the 1950s but found watching the machine (the IBM 650) do all the work dull. Maxwell had been an enthusiast for desk calculator machines, but stopped using even these in the later years of his work. (Grosch 1991, 46-47) Of course neither of these is an attack on the reliability of the computer.

It is interesting to note that specific features of computers can be a source of concern about the reliability of machines. Herbert Grosch and Martin Schwarzschild were two scientists associated with the Watson lab who had grave misgivings about the potential for error introduced by floating point computer arithmetic. The view was that, in fixed point form, errors in arithmetic would tend to lead to results that were “off scale”,
either much too large or much too small, and thus be easily identified. Whereas floating point might keep the order of magnitude properly scaled but have serious errors in the actual digits. Despite these worries floating point was an immensely popular feature on computers. (Grosch 1991, 120; Schwarzschild 1986, 20) In my research and interviews I was unable to find any evidence that Eckert shared or rejected any such concerns about reliability.

The lack of any clear new worries presented by the computer for Eckert and his contemporaries did not imply that reliability had ceased to be an issue. On the contrary, again and again Eckert and other researchers subject their calculations to careful scrutiny, either in elaborate error checking or by comparison with older theory. This practice predates the computer. For example Brown carefully checked his own theory against Hansen and Delaunay’s to try to ensure its accuracy. (Chapront-Touzé and Chapront 2000, 38) This long heritage of checking results represents one way that astronomy’s long history of computing allowed it to accept the reliability of computers automatically. Knowing that the prescribed checks were occurring guaranteed the new results the same status as the old ones.

7.4 Eckert as Trader on the Computer Frontier

Peter Galison has described the use of the computer as a site where diverse professions in science find common ground. In particular Galison describes the method of Monte Carlo computer simulations as a trading zone. Evoking the practice of sea traders in the age of the sail, he calls Monte Carlo simulations a pidgin, a primitive language, allowing diverse groups from different disciplines a common means of interaction. He also stated that Monte Carlo developed into a creole, a full fledged language, as it became its own discipline. (Galison 1997, 768-771) The same analogy seems to be tenably applied to the computer and its precursors more generally.
Eckert himself sought to create such sites of interdisciplinary commerce, and to some extent succeeded. Eckert’s 1930s work with punched cards had convinced a wide variety of groups, from Linus Pauling’s x-ray crystallographers to Los Alamos’s nuclear physicists, of the potential value of punched card machines to their various endeavours. Eckert pursued these connections at the Watson Bureau, in his 1940 book, in some of his writings in the 1940s and in his directorship of the IBM Watson Lab with its diverse research interests. Eckert even briefly dallied with the world of Monte Carlo in his never published second edition of *Punched Card Methods in Scientific Computation*.

Whether out of philanthropy or desire for new markets or both, IBM, and its president Thomas J. Watson, sought to promote the use of their machines in scientific computation. Eckert, in accepting IBM’s help, also took on responsibility to help ensure this happened. In this sense he was even more literally a trader. He traded his scientific and computational expertise, organizational skills, reputation and academic connections to IBM in exchange for use of their machines. His employment as head of pure science at IBM in 1945 made this relationship official. Eckert brought IBM not only his prestige and skills, but also his ethos of scientific research as an end in itself and academic freedom as a means to that end. While he was only one of many scientists to shape IBM’s research agenda, his early status suggests he left a real mark on the company in this regard.

Eckert is sometimes remembered as the designer of the SSEC. While he was given official responsibility for the project, his role seems to have been more a facilitator for his skilled staff, especially the duo of Rex Seeber and Frank Hamilton. Similarly the two other machines associated with Eckert, NORC and IBM 610, were not the direct work of Eckert, but he recognized the talent of those responsible and gave them the freedom to pursue such projects.

After the publication of *Faster, Faster* in 1954, Eckert’s efforts at informing a broader public in and out of the academic press ceased. While his directorship of the Watson lab must still have brought him into contact with a broad range of scientists, his published
work focused solely on astronomy. This change in focus may be due to a renewed focus on astronomy, increased administrative work or he may simply have recognized that widespread adoption of computing meant his proselytizing for computers was no longer necessary. In any case, his role as a trader in computer techniques had ended. The only exception seems to be his coworkers such as Harry Smith and Martin Gutzwiller. Smith would become a dedicated computer scientist. Gutzwiller would combine his knowledge of quantum mechanics with work in chaos theory and took inspiration in this from some of Eckert’s work and findings. (Smith MS no date; Gutzwiller 1989, 12)

Counterbalancing Eckert’s success at soliciting the support of IBM for his research were the commitments this created for him that blunted his research opportunities. However, unlike some other computer pioneers, he did not give up his original scientific research for the new possibilities of the computer field. In comparison two other prominent scientists turned computer designers, Howard Aiken and John Mauchly, gave up their scientific pursuits in favour of computers. Aiken became a professor at Harvard teaching and researching on the design and programming of computers. Mauchly became an executive in the computer industry. Like Eckert, their scientific interests and the difficulty of carrying out computations had led them to find better ways to calculate, but this search had led them away from their original goals. (Cohen 1999, 21-38, 201-214; Cortada 1987, 168-174)

In terms of Galison’s trading zone analogy, Aiken and Mauchly had learned the new creole language of computers and become permanent residents of that community. Eckert on the other hand stayed an opportunistic foreign trader and slowly drifted away from that hub of activity. The new skills required by computers and the opportunities they created meant there was a temptation among adoptees to abandon their original pursuits. While not irresistible or universal, this temptation is one of the clearest examples of how the computer can change an individual scientist’s research efforts.
7.5 Computer Experiments

Another observation by Galison about Monte Carlo simulation is that it has many properties of experiment. In particular, he points to issues of error analysis, stability, repeatability of the simulation and the sense in which the simulation seems to supply data rather than predictions. Others, especially, philosophers of science, have focused on computer simulations as a key instance of novel methods in science introduced by the computer. The novelty introduced ranges from the ability to utilize existing human capacity of visual analysis to the computer simulation as a site for the creation, deployment and modification of new sets of modeling techniques. Another issue is the ability to discover new phenomenon by computation. On the issue of the experimental character of simulation, philosopher Eric Winsberg adds calibration to Galison’s list of experimental properties. (Galison 1997, 730-735; Winsberg 2003, 110-111, 121-124; Humphreys 2004, 105-114)

Numerical integration of orbits, used by Eckert, matches, in most respects, the character of a model as such writers describe it. At each point in the simulation each planet has a calculated position, from which is calculated the mutual gravity and that is fed into the formulas to calculate the next set of positions. However, most would not include it as a computer simulation because it is relatively simple and resembles, in both form and output, older hand computation. Certainly Eckert rarely attempts to take advantage of the visual analysis of data or other such heuristics found in later machine presentations of data, the obvious exception being the graphs sometimes used in error analysis.

Despite this, I think that both Eckert’s work and other work in celestial mechanics, demonstrated how, not only computer work, but all complex computations, can take on some of the characteristics associated with experiments.

Error analysis was an important part of Eckert’s work. The errors of concern were not merely arithmetic errors, although these were of concern as shown by the extensive error checking methods. Beyond such simple checking, comparison of new theories with old was important, especially when they used entirely different methods of solution. One
of the reasons Eckert gave for creating a new analytical theory by the Hill-Brown method was comparison of such a solution with the one he and Smith found by the method of Airy. A comparison of two solutions with different mathematical bases could expose not only arithmetic, but systematic error. Such systematic error could not be detected in terms of arithmetic errors or a failure of two otherwise identical calculations to give differing results. In comparing theories, Eckert and others often gave the differences between the two calculations in terms of a standard error, as if comparing an observation or experiment with a theoretical prediction.

Stability of solutions in computation raises a possible source of systematic error or outright failure of a technique. Stability, in a technical sense of consistency of similar methods, was a background problem for celestial mechanics, at least since Poincaré had shown the potential high-sensitivity of results to initial conditions. This could lead to diverging results when attempting to solve the same problem by even slightly different means. Here again comparison with past theory helped check for divergences. Also, since many calculation techniques, such as Airy’s method, involved successive approximation, intermediate approximations could be compared to insure no divergences were occurring. In a case where a solution diverged before necessary accuracy was achieved, attempts would be made to eliminate the divergence, as when Eckert and Smith were forced to calculate terms to far higher precision than the desired final precision.

As a result of the method of error detection favoured by Eckert, reproducing a result was a key part of error analysis for him and other astronomers. The near inability to reproduce Brown’s computations exactly as he had done them meant it was difficult to establish what error to expect or how to even use the equations. This emphasis on reproducibility is also found in physical experiments, where the reproduction of results insures their reliability and generality.

I have argued that the computer made possible perfect reproduction of previous results with an ease and directness not seen before. Also, I think the emphasis on reproduction
of computational results parallels the drive to reproduce physical experiments. Galison in his analysis of computer experiments chose to emphasize failures of reproduction due to variations in machine used, coding styles, and problems with distribution due to logistics and property rights. Galison points to these local contingencies as characteristic of physical experiment. (Galison 1997, 733-734) Clearly code sharing and the like rely on the right habits being in place. Even admitting those limits, the effective extent of sharing was probably far higher than would have been feasible under the constraints of large scale hand computations. In terms of the local character of methods and results, pre-computer methods were even more difficult to reproduce. Whether this makes them more or less like experiments depends on the experiment.

Eckert’s method of error analysis points to his treatment of the results of computations as if they were experimental data. Despite this the rhetoric of calling computations “experiments” never emerged in Eckert’s work. Yet Eckert and others always spoke of the expected precision of the calculations as its accuracy and often had limited discussion of physical observations. More directly, in the numerical integration of the outer planets, the values of positions were not derived directly from observations but rather from the positions given by previous solutions. The sense was clear that an old predicted orbit for a planet that had been proven accurate by observation could be used as a source for actual positions. Part of this approach was pragmatic, such theoretical positions are capable of being given at any time you like and in a regular distribution, unlike actual observations.

Practices resembling calibration in computation predate the computer and are not unique to complex computer simulations. For example Brown and his assistants had to expend a great deal of effort to find the best values of the parameters to substitute into his lunar theory, thereby calibrating it to the world. A better example was the need to run the numerical integration of the outer planets several times, and adjust (calibrate) the initial conditions after those test runs to ensure the best solution. While this may be
very limited calibration of the solution compared to what is required for more complex computer simulations, they still establish the long precedent.

Winsberg’s idea of simulations as a developing toolkit of solutions tailored to a problem, captures something important about computer simulation techniques. Perhaps just as important, he recognizes that simulation techniques form part of a larger activity he calls “model-building” and so is aligned to activities that predate the computer. (Winsberg 2003, 118-124) The long slow advance of analytical theories for the Moon represents the development of such model-building tools. Innovations such as the Hill-Brown method represent not just an attempt to continue the same thing to another level of precision, but an attempt to find solutions fitted to the problems and short-cuts to avoid long computation. In this sense Eckert’s varied attempts to improve lunar theory constitute an attempt to sharpen existing tools or develop new ones to fit the problem.

Perhaps the key to why people make the analogy between computer work and physical experiment is psychological. The results of more direct theoretical reasoning can unfold out of a process of thought and argument, whereas experiment is to some extent received as a thing given by the outside world, a brute fact. Before a computation is run the outcome of the result is unknown and afterwards the reasons for the particular result may be obscure even to the calculator and so psychologically it has the character of a brute fact.

### 7.6 After the Computer

A simple way to interpret the role of the computer in science would be that context, methods, goals and results stay basically the same, there is just more of it getting done. While I found a great deal of continuity in Eckert’s work and in celestial mechanics more widely, clearly things changed and not just quantitatively. New institutions, like Eckert’s Watson Lab, sprang up creating new realities and possibilities for practitioners. New
methods appeared and old ones, like numerical integration, had to be revaluated. Even new goals were brought forth as with Eckert’s desire for aesthetic completeness rather than mere predictive accuracy in his final lunar theory.

The complexities of the subject mean that no simple extrapolation of past practice makes sense. There is no guarantee that a 1000 fold increase in speed will net any increase in accuracy of predictions due to the limits of measurement and mathematics. Despite ever better computer aided algebra, analytical solutions for planetary problems plateaued in accuracy and were overtaken by numerical integration. Yet this did not spell the demise of analytical solutions and indeed from 1968-1984, at the same time numerical integrations were achieving new heights of accuracy, 5 different analytical solutions of the Moon’s main problem were produced.\(^3\) Considering that from 1830-1960 only 4 lunar theories were completed\(^4\), the 1970s are a high water mark for analytical lunar theory.

The life and work of Wallace J. Eckert show that scientific horizons are indeed limited by computing power. He showed that with careful and judicious use of the available technology, these horizons can be pushed back further than others might think possible. For this very reason the change wrought by the computer continues to depend in part on the goals and methods of its users.

\(^3\)These were: Eckert-Smith’s solution, Deprit’s ALE., Henrard’s SALE, Chapront-Touzé’s ELP and the ELE. Note that the ELE actually represents at least two separately calculated solutions.

\(^4\)These were the solutions of Pontécomulant, Hansen, Delaunay and Hill-Brown. (Brown 1960, 245-246)
Appendix A

Glossary

Aberdeen Relay Calculator — Another name for the Pluggable Sequence Relay Calculator.

Accuracy — In this thesis the attempt is made to consistently distinguish accuracy from precision (see also Precision). This definition does not always reflect the historical actors’ use of the terms. For numerical terms accuracy designates the agreement between a measured or calculated value and the actual value. Put another way the smaller the size or range of allowed error the greater the accuracy. (Chandor et al. 1970, 24) So, a rope measured 5 metres long with an accuracy to the nearest metre may actually measure anywhere between 4.5 and 5.5 meters. A value can be given to greater precision than its accuracy. Accuracy will generally refer to physical accuracy. The agreement between an approximate computation and the exact value of the mathematical expression approximated will be called the precision or mathematical precision.

Airy’s Method — A method to derive a solution to lunar theory by successive approximation. An approximate solution is substituted into the differential equations of the Moon’s motion and deviations from the Newtonian ideal appear as residuals. The residuals are used as the basis to create a better solution via linear algebra. Eckert would make several attempts to use this method over the course of his career with various machine aids.

ALE — The Analytical Lunar Ephemeris (ALE) a solution to the main problem of lunar theory based on the method of Delaunay and completed around 1971. It is a
fully analytic solution with all constants left as literal terms until a solution is derived.

**Analog** — Pertaining to calculating machines, this adjective means that the machine works by making a direct physical analogy between the mathematical problem and the machine’s function and results are given as a measurement of some continuous physical property of the device. See also digital.

**Analytical Solution** — A solution, usually approximate, for the trajectory of a body given for all-time, by mathematical analysis, as a function in closed form. In the earlier twentieth century astronomy tended to denote a **Literal Solution** or sometimes a partially literal solution. Contrast with **Numerical Integration**.

**Aphelion** — Point in an orbit when the planet is farthest from the Sun.

**Apogee** — Point in an orbit when the planet (including the Moon) is farthest from the Earth.

**ASCC** — Stands for the IBM built Automatic Sequence Control Calculator, IBM ASCC, more commonly known as the Harvard Mark I. Howard Aiken and IBM collaborated to build this massive electromechanical machine. Also, known as a relay machine, but had older mechanical elements as well.

**Astrolabe** — A device for computing various stellar events, such as the time of passage overhead of reference stars, by use of projected an image of the celestial sphere etched on the device’s flat surface. A very old analog calculator that attained its iconic form due to Arab astronomers in the middle ages.

**Bell Mark V Calculator** — The Bell Mark V relay calculators, completed in 1946, were complex calculators fully controlled by a set of paper tape instructions and were produced by Bell Labs.

**Binary-Coded Decimal** — The encoding of numbers as groups of four bits with each group representing a decimal digit. Many IBM machines used a version of this in their basic representation of numbers.
Brown’s Theory of the Moon — E. W. Brown’s formulae (theory) giving the Moon’s position for all time. Brown found his solution using the Hill-Brown method, and so it is also called the Hill-Brown theory.

Brown’s Tables of the Motion of the Moon — Brown’s Tables of the Motion of the Moon, published in 1919, often abbreviated to Brown’s Tables. This volume was an aid to computing positions of the Moon according to Brown’s Theory of the Moon and was not a table of lunar positions. The book consisted of tables for the hundreds of trigonometric terms found in the series that made up Brown’s theory. Its apparatus included the accumulation of various terms and other short-cuts to speed computation and therefore was less accurate than the full theory. The task of summing the hundreds of terms found in the tables to solve the series was itself a significant task.

CPC — Abbreviation for the Card-Programmed Electronic Calculator.

Card-Programmed Electronic Calculator — Also called the CPC, this machine combined an IBM 604 as electronic arithmetic unit and IBM 402 or IBM 407 accounting machine to print output along with up to 48 registers of auxiliary relay storage.

Cowell’s Method — Cowell’s method refers to the finding of orbital trajectory by numerical integration, in astronomy, using rectangular coordinates. Often also refers to the specific formula or fundamental formula that Cowell and Crommelin derived in their work finding the path of Halley’s comet.

Deferent — In Ptolemaic astronomy the deferent is the main circular motion attributed to a planet centered at a point at or near the centre of the Earth.

Delaunay variables — Four variables, $F, l, l’$ and $D$, that can be combined in discrete multiples to form the argument of any of the trigonometric series in the equation for the motion of the Moon found by Delaunay. The form of solution in Brown’s lunar theory was such that all the terms in his equations for the Moon likewise had arguments made up by combination of the Delaunay variables. This was also the case with Eckert and Smith’s solution derived using Airy’s method.
**Differences** — For a function \( f \) calculated at some fixed interval the difference between the successive value is known as the first difference \( f' \), the difference of the first difference is the second difference \( f'' \) and so on. The method of differences uses the various differences to extrapolate the function. See also **Summation**.

**Differential Correction** — Various methods to refine or correct theory based on observational (or other) data using **equations of condition**.

**Digital** — Pertaining to calculate machines, this adjective means that the device works with discrete numbers as opposed to the continuous quantities of an **analog** machine.

**Double-Precision** — In a digital computer rational numbers are said to be stored in double-precision format if two words of memory are used to store each number. Precision in this case refers to the maximum precision i.e. the maximum number of digits that can be stored in the format. The actual precision of the number stored can be less than the maximum. (see also **Precision** and **Word**).

**Eccentric** — In Ptolemaic astronomy when the centre of the **deferent** in the theory was not the Earth it was said to be at an eccentric point.

**Eccentricity** — The eccentricity is a measure of the deviation of the ellipse from circularity, with \( e = 0 \) indicating a circle and \( e = 1 \) indicating a parabola. Also one of the standard parameters of an orbit in celestial mechanics.

**Ecliptic** — The circle in the sky that marks the Sun’s path during the course of the year. The constellations of the zodiac are on the ecliptic and the major planets and Moon all travel near the ecliptic.

**EDVAC** — The Electronic Discrete Variable Computer was designed as a successor to the ENIAC. The design ideas put forward in its initial conception, the von Neumann architecture, became the basis for most subsequent computer design.

**ELE** — The ELE or Eckert Lunar Ephemeris was the lunar theory, set of formula of
position, generated by Eckert, Bellesheim and Gutzwiller’s efforts to implement the Hill-
Brown method for solving the main problem of lunar theory on computer. The
Gutzwiller-Schmidt solution of the main problem is also called the ELE, but it was sub-
stantively different, even though it still implemented the Hill-Brown method.

ELP — The ELP or Éphéméride Lunaire Parisienne refers to various complete lunar
theories created by Michelle Chapront-Touzé and Jean Chapront, a husband and wife
team and used by the French Bureau des Longitudes as the basis for their astronomi-
cal tables, including Connaissance des Temps, starting in 1984. The method resembles
Airy’s method by using a successive approximation starting from an analytical solution
and then using a different method to achieve greater accuracy.

ENIAC — Abbreviation for Electronic Numerical Integrator and Computer, an early
large automatic electronic computing machine. This machine lacked the full flexibility of
a modern computer.

Ephemeris — An ephemeris is a compilation of daily positions of some celestial body.
The plural of ephemeris is ephemerides.

Epicycle — An epicycle was a motion posited in Ptolemaic astronomy for the planets.
The motion was an additional smaller circular motion on top of other circular motions,
such as the deferent and there could be epicycles on top of epicycles.

Equant — An equant construction in Ptolemaic astronomy posited a planet as moving
on a deferent around an eccentric point such that the deferent motion appeared uni-
form (constant) relative to a third equant point.

Equation of Condition — An equation that expresses a relation or condition that holds
if no error is present. For example an equation for the difference between an approximate
equation for the trajectory of a body and the observed trajectory. Solving these equations
therefore quantifies the error and can be used to fit parameters, or curves. This reduces
error relative to using uncombined or analyzed data. In most of the cases discussed in
this thesis the equations of condition are linear regression models. A linear model is one
where an equation for an independent variable is given as the linear sum of dependent
variables times numerical coefficients. In linear regression one derives approximate values of the numerical coefficients from the data.

**Equatorium** — Also known as a *volvella*, the *equatorium* were a class of complex analog devices for calculating planetary motions in a way that approximated ancient astronomical theory. The most prominent examples are known to come from medieval Europe.

**Fixed Point** — Fixed point is a machine storage format for numbers analogous to ordinary decimal notation. Computers will normally store a fixed number of digits or bits for any number stored (e.g. all numbers stored have 10 digits including zeroes). The decimal place is fixed (say after the 5th digit in storage) and calculations proceed on that basis. There may or may not be allowance at the hardware level for altering the assumed location of the decimal point. See also **Floating Point**.

**Floating Point** — Floating point is a machine storage format for numbers analogous to scientific notation. The numbers are stored in two parts, one gives the magnitude (e.g. in powers of ten for base 10 numbers) and the other part is made up of the actual digits (or bits if the number is binary). See also **Fixed Point**.

**General Perturbations** — Synonym of **Analytical Solution**.

**Harvard Mark I** — The popular name for the IBM ASCC.

**Hill-Brown Method** — The Hill-Brown method or solution to the main problem of lunar theory was originally put forward by George Hill. It begins with Hill’s variational orbit and is a solution in rotating rectangular coordinates. Brown used this method to construct his lunar theory. Thus Brown’s theory is also known as the Hill-Brown theory.

**HMNAO** — Her [His] Majesty’s Nautical Almanac Office of the British Admiralty, now part of the UK Hydrographic Office.

**Hollerith machines** — Herman Hollerith developed the first commercial punched card calculating machines in the 1890s. A Hollerith machine was a machine that used
the punched card system, especially a machine based on Hollerith’s designs and patents.

**Hollow Moon Paradox** — A description given to Eckert’s suggestion that an unexplained deviation in the Moon’s node from lunar theory could be explained by positing a concentration of mass near the surface of the Moon.

**IAS** — IAS is an abbreviation for the Institute for Advanced Study at Princeton University.

**Institute for Advanced Study Computer** — A computer designed by von Neumann and built at the Institute for Advanced Study. Also called the IAS computer and IAS machine.

**IBM** — IBM originally stood for International Business Machines, a name adopted by the Computing-Tabulating-Recording company in 1924. IBM was the dominant provider of punched card equipment in North America. IBM’s machines included those developed by Herman Hollerith and all their punched card machines were considered to be derived from his work.

**IBM 402** — The IBM 402 tabulator was developed by IBM and was first offered for rental in 1948.

**IBM 407** — The IBM 407 tabulator was the last major electromechanical machine developed by IBM, had a novel printing mechanism and was first offered for rental in 1949.

**IBM 601** — The IBM 601 multiplying punch, also called the multiplier, this machine released in 1931 performed multiplication and punched the result on the inputted punched card.

**IBM 602-A** — The IBM 602-A calculating punch was IBM’s last electromechanical multiplier and was capable of performing operations consisting of up to 12 steps on a single punched card.
IBM 603 — The IBM 603 electronic multiplying punch was a machine that performed multiplication on punched cards via vacuum tubes. It was the first commercial machine to use electronics for arithmetic and was first offered for rent in 1946.

IBM 604 — The IBM 604 electronic multiplying punch performed multiplications and many other functions on punched cards via vacuum tubes. It was capable of performing operations consisting of up to 60 steps on each punched card. It was introduced for rental in 1948.

IBM 610 Auto-Point Computer — The IBM 610 was an electronic machine intended as a personnel computer, operated by one person using a keyboard, but was limited in complexity and speed and released in 1957.

IBM 650 — The IBM 650 was an early IBM computer introduced in 1954 based around a rotating magnetic drum memory. It was a relatively small machine and hundreds were produced.

IBM 701 — The IBM 701 computing plant, or IBM 701 computer, was IBM’s first commercially available computer. First offered for rental in 1952, this large scientific computer’s design was based on the design of the IAS computer.

IBM 704 — The successor to the IBM 701, a large scientific computer built by IBM in beginning in 1955.

IBM 1620 — The IBM 1620 was a relatively small and inexpensive computer released in 1959.

IBM 7090 — The first large transistorized machine built by IBM, it was first delivered in 1959.

IBM 7094 — A large powerful transistorized machine designed for scientific use and released in 1962.

ILE — Abbreviation of Improved Lunar Ephemeris for the years 1952-1959, the compu-
tation of the position of the Moon based directly on Brown’s theory by Eckert and his assistants between 1948-1952. The calculation actually extended to 1972 and for part of the 1960s this calculation was the basis of the published ephemerides. This replaced the use of the more approximate method of calculation that made use of Brown’s Tables.

**Inclination** — The angle between the plane of an orbit and the ecliptic. A standard parameter of an orbit designated $i$.

**Interpolation** — The act of finding the values of a function or curve between two previously established values or points. The interpolation uses some approximate numerical computation formula to achieve this. A linear interpolation for example assumes that between the two known point the function is a straight line and so as a formula has the form $y = y_0 + x(y')$.

**JPL** — Acronym of the Jet Propulsion Laboratory.

**Keypunch** — A keypunch punched holes in punched cards and may have had additional features such as verifying the correct holes were punched.

**Libration** — Literally a libration is a rocking back and forth motion. With respect to the Moon this refers to the Moon’s tendency to change its orientation towards the Earth slightly over time, back and forth.

**Literal Solution** — An Analytical Solution for the trajectory of a body where all of the coefficients of the solution are left as literal constants (eg. the $a$ in $ax$) as opposed to substitution of numerical values. Sometimes also used for partially literal solutions where some but not all of the coefficients are left as literal constants. Contrast with Numerical Solutions.

**Main Problem of Lunar Theory** — The main problem of lunar theory consists of finding the a solution to the equations of motion of the Moon that accounts for the interactions between the Earth-Moon-Sun system when treating the bodies as point masses. A complete lunar theory requires the consideration of other factors such as planetary perturbations.
Micrometer — Micrometers are based on the principle of using a large ratio of torque to turn a relatively large motion of a screw into a very small motion of some measuring device, calipers for example. In astronomy micrometers were used to measure angular distances with high accuracy by controlling the position two parallel threads or wires superimposed over the celestial objects in a telescope. Precursors to the astronomical micrometer were in use by the mid-17th century.

Monte Carlo Simulation — A broad range of numerical methods that utilize a stochastic (random) element to generate solutions to a mathematical problem. Applications range from the direct simulation of physical phenomenon such as atomic chain reactions to the integration of differential equations such as the Schrödinger Equation.

Multiplier — In terms of punched card machines, a multiplier is a machine that multiples numbers inputed on a punch card, often punching the result. Examples of the multiplier include the IBM 601, IBM 602A, IBM 603 and IBM 604.

NAO — Nautical Almanac Office of the United States Naval Observatory.

Node — The nodes of an orbit are the two points where it crosses the ecliptic. The ascending node Ω is used as a standard parameter of an orbit, it is the point where the body passes through the ecliptic traveling towards celestial north (up).

NORC — Naval Ordnance Research Calculator, a computer built for the Navy by the IBM Watson lab, finished and dedicated in 1954.

Numerical Integration — Finding the values of an integral using starting numerical values and a algorithm for extrapolation based on the equation to be integrated. In astronomy it is usually used to find the positions of a planet at fixed intervals of time over some fixed period of time, by integrating the differential equations of Newtonian gravity. Also, called the method of Special Perturbations and mechanical quadrature. Contrast with Analytical Solution.

Numerical Solutions — A solution for the trajectory of a body where numerical val-
ues have been substituted for some or all of the coefficients before deriving the form of the solution. Sometimes also used to describe **Numerical Integration**. Contrast with **Literal Solution**.

**Perturbations** — The perturbations of a planet’s motion are its deviations from some simple ideal such as a Kepler ellipse. Often the perturbations are identified in terms of their source. So planetary perturbations are the motions attributable to the gravity of the (major) planets. The perturbations may refer to actual physical motions or the terms in an equation describing the motion.

**Perigee** — Point in the orbit of a planet where it is nearest to the Earth.

**Perihelion** — Point in the orbit of a planet where it is nearest to the sun.

**Plug Board** — A board of pluggable wires, like an old fashioned telephone switchboard, that control the operation of a **punched card** machine.

**Pluggable Sequence Relay Calculators** — A **relay calculator** built by IBM for the Aberdeen ballistics calculation group in 1944. Two were also provided for the IBM Watson laboratory at Columbia in 1946.

**Precision** — For numerical terms precision designates the extent a number (in terms of number of digits, for example) is consistently specified by a measurement or as the output of a calculation. It indicates both the limits of the initial information and in the case of calculated values further losses due to the effects of mathematical operations, rounding errors and the like. The precision need not include all the digits written, for example the number 200 may be precise to only one digit (2) with the two zeroes indicating the magnitude. Precision is independent of physical **accuracy**, except that it sets an upper limit on accuracy. For example, a digital thermometer consistently showing a temperature of 11.2°C gives three digits of precision or one can say it is precise to the first decimal place. Such a thermometer can not be accurate to the second decimal place, and it need not be accurate to the first decimal place. See also **double-precision**.

**Project Hippo** — A project carried out on the **SSEC** that involved solving a very large
hydrodynamics problem for the Atomic Energy Commission.

**Prosthaephaeresis** — A technique for performing multiplication by taking advantage of trigonometric identities and so replace long multiplications with a simple set of table look-ups and a few simple arithmetic steps. A precursor to multiplication by logarithms.

**Punched Card** — A rectangular card with holes punched in it, usually to represent numbers. Beginning in the late 19th century, punched cards were used in conjunction with various machines to perform various simple arithmetic and sorting operations, such as the compilation of the U.S. census data. Punched cards became an early input and output medium for electronic computers.

**Quadrature** — Quadrature has two meanings in this thesis. One meaning of quadrature is the attempt to find the area of a figure using inscribed polygons and this meaning was extended by mathematicians under the term mechanical quadrature to include all discrete numerical integration techniques. In astronomy quadrature refers to the point in a body's orbit when the angle between that body and the Sun measured from a third body is a right angle. In the case of the Moon, its quadrature equates to the half-Moon phase of its orbit.

**Relaxation Procedure** — Various methods of solving linear algebra equations by successive approximation through the systematic modification of a trial solution.

**Relay** — An electromagnetic relay uses an electromagnet to switch (open or close) a circuit and thus manipulate electric flow to other components, including other relays. They were used extensively in high-end calculating machines in the 1940s.

**Reproducer** — The high-speed reproducer or automatic reproducing punch was a punched card machines that rapidly copied punched cards.

**Residuals** — Generally used to describe any error, the residual difference between a prediction and measurement. In Airy's method for generating an improved lunar theory the first step involves finding the deviations, from Newtonian theory, of the best current theory by substituting it into the differential equations of Newtonian Gravity and seeing
what terms remain. These terms are called the residuals and these are used as the basis to fit a new solution to the problem.

**SALE** — The Semi-Analytical Lunar Ephemeris, a solution to the **main problem of the lunar theory** that used Hill’s **variational orbit** as a starting point, but proceeded in a different manner than the Hill-Brown method.

**SCAT programming system** — The SHARE compiler-assembler-translator programming system was an early programming language for the SHARE operating system (SOS).

**Sexagesimal** — A sexagesimal number system, such as the Babylonian number system, has a base of 60.

**Secular Acceleration** — Ongoing acceleration of a celestial body that leads to a permanent increase or decrease in its speed is called secular acceleration. This is opposed to the periodic accelerations that bodies experience that are ultimately canceled out by contrary accelerations over the course of time. The terms accounting for this acceleration in the equations describing the motion are called secular terms.

**Semi-Major Axis** — In an ellipse the major axis is the larger of the two axes of symmetry. The semi-major axis is half this distance. It is also the average of the shortest and longest distance of a body in an elliptical orbit from the focus of attraction (eg. the Sun).

**Sorter** — In terms of punched card machines, a sorter refers to a machine that sorts cards into up to 13 groups based on where in a single column a number has been punched.

**SOS** — The SHARE Operating System was developed by the IBM user group SHARE. One of the earliest operating system that controlled various aspects of input, output and routine computer operation. It included integrated programming features such as the **SCAT** language.

**Southwell Relaxation Procedure** — A specific **relaxation procedure** named after its inventor Richard Vynne Southwell a mid-20th century British aerodynamics engineer.
**Special Perturbations** — Synonym of **Numerical Integration**.

**SSEC** — Selective Sequence Electronic Calculator, IBM’s first large-scale automatic calculator to use electronics for arithmetic.

**Stellar Aberration** — The stellar aberration is the deviation in the apparent angular position of a star due to the aberration of light. The aberration of light is due to the relative motion between the source of light and its receiver and the finite speed of light. Also, called simply the aberration.

**Stored-Program** — The stored-program concept refers to the idea of storing the instructions and data for an automatic calculator in the same format allowing instructions to be changed based on the data via arithmetic manipulation. An important basic feature of modern computers.

**Summary Punch** — A device developed by IBM to supplement their tabulators. It allowed numbers stored in the tabulator to be punched onto cards for future use.

**Summation** — A term derived from a function \( f \) at fixed intervals. A summation is the inverse of the **difference** such that the difference between the two consecutive first summations \( f' \) is the original function, likewise the difference between two consecutive second summations \( f'' \) is the first summation. Summations are used in Cowell’s method of numerical integration.

**Syzergies of the Moon** — Syzergy is simply a term for when three celestial bodies sit on the same line. The syzergies of the Moon’s orbit are the points where the Earth, Sun and Moon lie on the same line. When the Moon is in syzergy and on the same side of the Earth as the Sun this is conjunction and when the Sun and Moon are on opposite sides it is opposition.

**Tabulator** — A punched card tabulator was a device that in general could only add, total, sub-total and print numbers as inputed on punched cards and controlled via a plug board. See IBM 402 and IBM 407 for specific examples.
Variation — The variation is a motion of the Moon, part of its periodic deviations from perfect uniform circular motion discovered by Tycho Brahe.

Variational Orbit — The orbital trajectory postulated and first analyzed by Hill. This periodic orbit is a simple periodic motion except for the addition of the motion known as the variation. See also Variation.

Volvella — Volvella is another name for a equatorium.

Word — In computer terminology a word is the standard unit of storage that usually contains one number of data or one instruction. Early computer had many different word sizes, some even had variable word sizes. The concept is analogous to the byte in modern personnel computers, originally a byte consisted of 8-bits.

$x_n$ — Usually $x_n$ refers to the x coordinate of the n$^{th}$ body or the like. However throughout their solution of the Lunar problem by Airy’s method Eckert and Smith use the symbol $x_n$ to refer to successive approximations of the equations of lunar position. They sometimes call these the coordinates. $x_0$ refers to Brown’s equations of position from his original lunar theory.
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